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Certainly it is excellent discipline for an author to feel that he must say all he has to say in the fewest possible words, or his reader is sure to skip them; and in the plainest possible words, or his reader will certainly misunderstand them. Generally, also, a downright fact may be told in a plain way; and we want downright facts at present more than any thing else.—RUSKIN.

Original Communications.

THE NATURE OF FEVER.*

BY JOSEPH IUTZI, M.D.

There are few subjects of greater interest to the physician than that of fever, as diseases unaccompanied by fever during some part of their course are exceedingly rare. But little was known of the real nature of fever until within the past few years. Liebermeister and Leyden claimed that there is increased heat-production in all fevers. Traube, on the contrary, held that heat-retention played the more important part. It was reserved, however, for American observers to show the errors of Liebermeister's experiments. First among these were Walton and Witherle, who demonstrated that heat-retention played an important part in the abnormal rise of temperature. But the most thorough and conclusive experiments were made by H. C. Wood.

Before proceeding to attempt to summarize our knowledge of

* Read before the Wayne County (Ind.) Medical Society.

this complicated subject I will attempt to define the essential symptom of fever. In a typical fever there are four different groups of phenomena: (1) Acceleration of the heart's action and disturbances of circulation; (2) Disturbances of the nervous system; (3) Disturbances of nutrition and secretion; (4) Elevation of bodily temperature. These several phenomena may bear one of two relations to each other; one may exist as the cause of the others, or one and all may be the result of a still deeper cause.

Let us examine them in the order named and see if we can determine what their relation is. If by inducing one symptom all the others should follow, we would conclude that that symptom is the cause of the others. It is well known by clinical observation that the heart's action may be accelerated and the equilibrium of the circulation disturbed without any appreciable disturbances in innervation and nutrition or elevation of temperature. Among the nervous disturbances of fever we may name delirium, stupor, coma, convulsions, and paresis, all of which frequently do occur without elevation of temperature or marked disturbances of nutrition or the circulatory system. It is true that disturbances of nutrition may cause elevation of bodily heat, but this is so out of all proportion to the nutritive disturbances and so frequently absent that we may at once exclude it from being the essential symptom. There is only one other symptom which needs consideration—that of elevation of animal temperature. If it can be shown that we can produce all the other symptoms by simply producing an abnormal degree of elevation of the animal temperature, we may safely infer that this is the essential symptom of fever. To settle this question Dr. H. C. Wood made some experiments, from which I select the following. These experiments are copied from his work on Fever.

Exp. 1. Exposed a two-thirds-grown rabbit, in a box covered with glass, to sun's rays. 1 P.M. temperature in rectum 104.5° F.; temp. of box 120° F. 1:15 P.M. temp. 106.5°; respiration very hurried. 1:30 P.M. temp. 109.5°; has convulsive attacks, in which he jumps and kicks with hind legs with great fury. 1:45 P.M. temp. 112°; seems

very weak and relaxed; breathing 220 a minute; lies on side; slobbers greatly. 2:10 P.M. temp. of box 120°; rabbit on side, very weak, gasping; squealing faintly at intervals. 2:15 P.M. temp. 114.5°; perfectly unconscious; lies relaxed and motionless on the ground in the shade. 2.20 P.M. only gasping at long intervals; heart still beating, although somewhat irregularly, yet pretty steadily and forcibly. 2:21 P.M. dead.

Autopsy. Heart—right side and left auricle full of blood; not contracted. The heart made a few very imperfect and feeble attempts at beating when it was cut across. Blood coagulated with great rapidity and firmness; alkaline. Brain not congested. Muscles all failing to show the slightest sign of contraction under the strongest faradic current, except some of the leg-muscles, which contracted very feebly and only when the current was very intense.

EXP. 4. A moderate-sized dog. 1 P.M. put in the hotbox, artificially heated; 1:15 P.M. rectal temp. 106°; 1:30 P.M. rectal temp. 110; 1:40 P.M. rectal temp. 110.75°; just dead.

Autopsy. As soon as respiration ceased body was opened. Heart still beating, gorged with dark blood. Veins full of dark blood. Blood on being shaken in test-tube rapidly clotted, and slowly changing in color to an arterial hue.

EXP. 5. Adult pigeon. 11:40 A.M. rectal temp. 109°; temp. of box 130; 11:45 A.M. respiration very weak. 12 M. has been unable to stand for some time; has been semi-unconscious; convulsion followed by persistent opisthotonos. 12:2 P.M. rectal temp. 120°; dead. Respiration certainly ceased before heart's action. Rigidity came on almost before heart ceased to beat. Thorax opened as soon as heart ceased beating. Heart found rigidly contracted. Muscles acid.

We are not justified in making experiments of this kind upon man, but we sometimes find them made by nature in the form of sunstroke, or, more properly, thermic fever. Dr. Bonnyman, in his very graphic account of sunstroke, shows how much alike are this disease and other forms of fever. He writes, "Where premonitory symptoms show themselves they are sometimes well marked. Those usually observed are inaptitude and disinclination for any exertion, drowsiness or a desire to sleep, vertigo, headache, and slight confusion of ideas. The patient feels weak, sighing frequently. The appetite is gone, thirst is increased, and the bowels are constipated. The symptoms be-

come aggravated, and the patient passes either into a state of profound coma, or symptoms of the first form of the malady are complained of, viz. distressing headache, with a feeling of weight and heat in the occiput, tightness, distension, and throbbing in the forehead and temples, anxiety at the precordia, nausea, and a disposition to vomit. A sense of sinking or of insupportable weight or uneasiness is referred to the pit of the stomach, and a feeling of horror of impending calamity, with a tendency to weep, is experienced. The breathing is natural, or slow and sighing. The face is natural or somewhat flushed, eyes bright, pupils either natural or somewhat contracted. The skin is very hot and dry, the pulse is full and accelerated, tongue white, thirst intense, bowels confined, the urine suppressed. If the symptoms persist, tetanic convulsions suddenly appear, and the patient lapses into the second or severer form of the disease."

The condition of the blood in thermic fever so nearly resembles that found in malignant fevers that some eminent physicians believed it to be due to some poison in the blood.

By the foregoing experiments it is shown that when the animal temperature is artificially raised symptoms are produced which are similar to those we find in ordinary fever, and the intensity of the symptoms is directly proportional to the elevation of the animal temperature.

Dr. Wood made a number of experiments to show the effect of heat upon the brain. The head was surrounded with an india-rubber bonnet and hot water allowed to flow through it. The following phenomena were produced, viz. hurried respiration, very rapid pulse, severe convulsions, partial unconsciousness which became total. The rectal temperature was raised, which, however, was not as high as that of the brain, that of the brain ranging from 114° to 117° F. In one experiment the animal was brought to the verge of death. When cold water was poured over the head the animal immediately revived, and the next day was well except the local injury about the head. From these experiments Dr. Wood draws the conclusion that a temperature of the brain of 113° to 117°, if maintained, is capa-

ble of producing death in a short time in mammals by arrest of respiration.

It may be objected here that the convulsions and other nervous disturbances are due to congestion of the brain, and not to the heat absorbed. The objection will not hold good when we remember that epileptic convulsions are now held by the best authorities not to be due to congestion. The profuse hemorrhage produced by opening the skull did not afford relief to the convulsions. "Abstraction of heat by pouring cold water over the head produced immediate relief."

If the heart of a living frog be cut out and a gradually increasing degree of heat applied to it, the heart will beat faster and faster until a certain limit is reached. If we stop short of this limit and allow the heat to slowly dissipate, it will diminish in the frequency of its pulsations until it stops. Panum has experimented on the heart of the rabbit with the same results. Leibermeister sums up his observations on the action of the heart in its relation to temperature in the following table:

Temperature (Cent.), . .		37°	38°	39°	40°	41°	42°
Pulse	Maximum, . . .	124	148	160	158	160	168
	Minimum, . . .	45	44	52	64	66	88
	Mean, ...	71.6	88.1	97.2	105.3	109.6	121.7

It will be seen that the maximum and minimum frequency of the pulse are wide apart for the same degree of heat, but there are so many factors entering into the production of an accelerated pulse-rate that uniformity can not be expected. But still, taking the average of a large number of cases in connection with the experiments above indicated, we may safely conclude that the frequency of the pulse rises, other things equal, with the rise of the temperature of the tissues of the heart.

From the foregoing it is evident that heat applied to the brain and heart will produce those functional disturbances in these organs that we find in fevers, and the amount of disturbance is in direct ratio to the increase of heat. But if increased heat is the cause of the other phenomena of fever, then the abstraction of heat will be followed by a disappearance of the

accompanying symptoms, provided the temperature did not rise to such a degree or continue long enough to produce permanent nutritive changes.

In the seventeenth experiment Dr. Wood put "a young rabbit in a glass box. In twenty minutes he was apparently totally unconscious, having passed through all the ordinary symptoms. He was now taken out and put in a bucket of water. The temperature rapidly fell to the normal, that of the water rising two degrees, and consciousness was restored at once. He was very weak, but was next day apparently as well as ever."

There are three other experiments given by the same author confirmatory of this one. In some of these the febrile symptoms all improved on the abstraction of heat, although the nutritive changes were so profound that the animals subsequently died.

It is well known that in hyperpyrexia due to any cause the abstraction of heat is followed by improvement of all the symptoms. Viewing the foregoing experiments from every aspect, the conclusion will be forced upon us that the essential symptom of fever is abnormal elevation of bodily heat. This being true, the subsequent portion of this inquiry will be greatly simplified. It will only become necessary to inquire into the effects of abnormal elevation of the animal temperature artificially produced and apply them to fevers.

Let us now inquire into the manner in which the animal mechanism controls the production and dissipation of heat.

In 1837 there came under the observation of Sir Benjamin Brodie a man in whom the spinal cord was severed, whose temperature rose in a few hours to 111° F. This induced him to make some experiments on animals in which he divided the spinal cord. He found that in many instances the temperature rose above the normal. Brodie was followed by Bernard, Schiff, Chasset, Binz, Naunyn, Quincke, Rosenthal, and others. Dr. Wood found when the cord was cut in the lower cervical region in *small* animals the temperature fell. If the temperature surrounding the animal was much below its normal bodily heat

this fall was permanent. If the animal be well wrapped in cotton or wool, and if the temperature is near the normal temperature of the animal, the fall is succeeded by a rise above the normal in from a few minutes to a few hours, so that the animal dies in a state of fever. To determine whether the rise of temperature is really due to section of the cord, Naunyn and Quincke made some experiments which are apparently crucial. They put the animal in a warm box for several hours without observing any rise in the animal's temperature. They now severed the spinal cord and replaced the animal in the box, when the temperature rose considerably above the normal in a very short time. They now took an animal, opened the spinal canal, but left the cord intact, and put the animal in a warm chest for ten hours. At the end of this time they found that the bodily temperature had risen only $\frac{6}{10}$° F. The following day the cord was divided. In twenty minutes the temperature fell nearly 1°, but it rose 3° during the following hour and twenty minutes, when the animal died.

Wood made several experiments in which he did not always find the temperature rise as fast after section of the cord as before, when the animal was surrounded with hot air. According to him, "the stronger the animal the more probability there is of an excessive rise after the division of the spinal cord."

"Is the first fall of temperature due to lessened production or to an abnormal throwing off of bodily heat?" To solve this question Dr. Wood made a number of elaborate experiments which are models of scientific accuracy. The calorimeter used and the calculations made are so complicated that they can not be described here. Calculations were made as to the heat given to the air, heat expended in vaporization, heat given to calorimeter, etc. Calculations were also made as to carbonic acid.

In a series of seven experiments in which the spinal cord was severed in various regions he found that "section of the cord is followed by a decided increase in the giving off of animal heat, and that the amount of increase is in direct proportion to the nearness of the section to the brain, provided respiration is not

seriously interfered with." In two other experiments, however, he found that heat-dissipation was diminished after section of the cord. In one of these a spitz dog with a very thick coat of long hair was used, and in the other the cord was severed low down.

We may here very appropriately address ourselves to the cause of the increased loss of heat. Tscheschichin found that after section of the cord the temperature of the interior of the body falls more rapidly than that of the exterior. Section of the cord paralyzes the vasomotor nerves and dilates the blood-vessels, thus making the communication between the outer and inner parts of the body more free; consequently there will be greater equilibrium between the temperature of these parts. Animals of the class mammalia have a mechanism by which they can control the loss of the bodily heat. In those having perspiratory glands this is very evident. But in such animals as the dog, the cat, etc. there are no perspiratory glands. In these the heat is regulated by the respiration and by the peripheral circulation. If heat is to be retained it is done by the contraction of the superficial capillaries. When the vasomotor nerves are paralyzed by section of the cord the capillaries can not contract, and the loss of heat is great. In the spitz dog above mentioned the increased heat-dissipation did not occur, and was no doubt due to the great thickness of its outer covering.

Is this increased heat-dissipation permanent, or does it last for a short time only? To determine this, Dr. Wood made an experiment which he sums up as follows:

Before section of cord, hourly dissipation of heat,	204.75
Forty-eight hours after section, hourly dissipation of heat,	123.40
Diminished hourly dissipation of heat in second period after section of cord,	81.35
Immediately after section, hourly dissipation of heat,	295.15
Forty-eight hours after section, hourly dissipation of heat,	123 40
Diminished hourly dissipation in second period as compared with first period after section,	171.75

This experiment, which is confirmed by others, shows that

the heat-dissipation is usually first increased and afterward diminished.

Another question that presents itself is this: Is the primarily-increased dissipation of heat accompanied by increased or diminished production of heat? As long as the dissipation is equal to the production of heat the temperature remains the same. When heat-dissipation is greater than heat-production there is a draught on the reserve heat, and the temperature falls. But without giving the complicated analysis which Dr. Wood makes of his nine following experiments, I will content myself by giving his conclusions. "Section of the spinal cord above the origin of the splanchnic nerves is usually followed by an immediate and very decided increase in the amount of heat dissipated, and also by a decided lessening of the amount of the heat produced." This diminished production of heat is due to vasomotor paralysis, and is probably accomplished in the following manner: General vasomotor paralysis produces dilatation of the capillaries, allows the heat to be rapidly dissipated, retards the circulation, and thus lowers the temperature. Lowered temperature and sluggish circulation check the retrograde tissue metamorphosis, and lessened tissue-change is accompanied by lessened heat-production. The vasomotor center is situated in the upper part of the medulla oblongata. "The test of the integrity of the vasomotor system is the effect of galvanization of a sensitive nerve upon the blood-pressure after the animal has been quieted by woorara and the pneumogastrics cut." If the blood-pressure rises the vasomotor system is not paralyzed. If section is made between the medulla and pons the temperature usually rises very rapidly and very decidedly, provided that the vasomotor center is not interfered with or the animal exhausted from hemorrhage.

We now come to the interesting question, Is there a center somewhere in the cerebro-spinal axis that regulates the production and dissipation of heat? It has already been pointed out that if section be made between the medulla and pons, thus severing entirely its connection with the brain, there will be a dis-

turbance in the natural balance between heat-production and heat-dissipation, and the result is abnormal elevation of bodily heat. As the vasomotor system remains intact in this operation, it can not be considered capable of itself to regulate normal heat-production and heat-dissipation. We naturally infer that by this operation some influence is cut off that kept the proper balance between production and dissipation of heat. Either the brain as a whole or some part of it has a controlling influence over the production of heat. The vast majority of injuries to the brain-substance has no appreciable influence upon thermo-genesis, but we do occasionally meet with cases of injury to special parts of the brain in which there is a marked increase in the bodily temperature. The brain as a whole therefore does not regulate animal temperature. We must therefore seek for some special center or centers.

Bastian states that in apoplexy of the pons the temperature of both sides of the body steadily rises till at the time of death it may attain 109° F. or even 110° F. He also states that in hemorrhage into an optic thalamus the temperature of the paralyzed limb may be 1.5° to 2° higher than the sound limbs. Dr. Remy reports a case (in Bull. Soc. de Anat., Paris) of hemorrhage into the optic thalamus which came under his observation about a month after the attack. The thermometric readings were as follows:

November 9, . . .	Right hand, 32.9° C.	Left hand, 36° C.
	Right elbow, 34.9° C.	Left elbow, 35.3° C.
	Right axilla, 37.1° C.	Left axilla, 37.3° C.
December 1, . . .	Right axilla, 37° C.	Left axilla, 37.2° C.
	Right elbow, 35.6° C.	Left elbow, 35.8° C.

I will also append the following cases in which there was hemorrhage into the pons:

Nunneley—Head at first alone hot; later, whole surface.

Alexander—Cross paralysis; fever only slight; temperature for a while 101.4° F. in right, 102.2° F. in left axilla; clot in right lower half of pons, not extending beyond median line.

Johnson—Temperature 1° higher in popliteal space of paralyzed side; lower on sound side; cross paralysis; no autopsy.

Huchard—One hour after attack temperature of right side 36° C.; left side 35.6° C.; right side paralyzed in one and a half hours after attack; large clot in left side of pons.

In the table from which I selected the above there are other cases in which the temperature was normal or subnormal, and others in which no mention is made of temperature. Further observation is needed in similar cases.

It is held by some that the increased production of heat is due to an irritation of the hypothetical heat-center, while others claim that it is due to paralysis. I shall not discuss the arguments for and against the irritation theory, but shall simply state that the weight of evidence which appears to me conclusive is in favor of the theory of paralysis. If it is true that paralysis or paresis of the heat-center is followed by increased heat-production, irritation of the center will diminish heat-production. It is therefore an inhibitory heat-center.

Thus far I do not claim that the existence of a heat-center has been proved by any thing contained in this paper, but that its existence has been made exceedingly probable. There is, however, experimental evidence which is conclusive on this point. If an inhibitory heat-center exists, destruction of it will be followed by increased heat-production. In one of his experiments Dr. Wood injected with a hypodermic syringe three minims of strong aqua ammonia into the pons of a dog. At the autopsy it was found that the cerebellum was extensively destroyed, as well as the upper fourth of the pons. The medulla was not injured. The result of calorimetry was as follows:

Hourly production of heat before injection, ...	34.65
Hourly production of heat after injection, ...	79.49
Hourly increase of heat-production after injection, ...	44.84

A gain of nearly one hundred and thirty per cent.

Eulenbeg and Landois found that when they destroyed a certain spot in the vicinity of the sulcus cruciatus of a dog the temperature of the extremities of the opposite side rose almost immediately. The difference of the temperature of the feet of the two sides varied from 2.7° F. to 23.4° F. They assert also

that when this region was irritated by electricity the extremities grew colder. The elevation of the temperature persisted for from a few days to three months, and then disappeared. These experiments are in accord with experiments made by Hitzig. Dr. Wood also investigated this subject. He destroyed portions of the brain with hot irons, caustic injections, etc. He made twenty experiments, on which he remarks as follows: "In looking over these tables it will be seen that there are fourteen experiments in which one or both of the first convolutions were injured immediately behind the sulcus cruciatus, and six experiments in which other portions of the brain were alone affected. In not one of the latter was there increased heat-production worthy of any notice immediately following the brain injury, while in thirteen of the fourteen experiments compromitting the so-called 'Hitzig region' there was a decided increase in the yield of heat. Of the thirteen consonant experiments, in seven there was one and in six there were both of the first convolutions injured. In the latter set the increase of heat-production was, reading the experiments as they are arranged in the table, about thirty-five, seventy-four, thirty-six, twenty-seven, sixty-five, and forty-seven per cent. In the experiments in which there was only one center injured the increased production of heat was ten, twenty-two, eleven, fifteen, thirty, and twelve per cent. The average increase of heat-production was therefore forty-seven per cent when both sides of the brain were affected, and seventeen per cent when only one side was compromitted. When to this relation is added the fact that in twenty experiments the results were uniform, except in one instance in which the brain was deluged with blood from a wounded sinus, it is difficult to resist believing that in the dog *destruction of the brain region* known as the first cerebral convolution posterior to and in the vicinity of the sulcus cruciatus is followed by an increase in heat-production. It may be noted that in several of the experiments, in which other portions of the brain than Hitzig's region were destroyed, there was a very decided fall in the heat-production. A plausible explanation of this fall is to be found in the suppo-

sition that in these instances the wounds of the brain were sufficiently near the Hitzig region to irritate it."

To test the correctness of this hypothesis Wood made two experiments. He trephined the skull over the Hitzig region, dissected off the membranes, and filled the orifice with salt. In each case there was a reduction of the hourly heat-production of twenty-two per cent.

Dr. Wood concludes from the twenty-two preceding experiments that "destruction of the first cerebral convolution posterior to and in the vicinity of the sulcus cruciatus is followed at once by a very decided increase of heat-production, while after irritation of the same nervous tract there is a decided decrease of heat-production."

As the destruction of this region is in all probability temporary in its effect on heat-production, it can not be looked on as the center sought for, but is very likely in some way connected with it. Wood thinks "the probabilities are that the calorific centers are situated in the pons, and that the power of the first convolution depends on habitual coaction. Thus volition may habitually use the upper cortex spoken of in starting the machinery of muscular movement, and along with this machinery, or even as a necessary part of it, that of heat-production may also be moved."

We now come to the old question, "Is the abnormally high temperature of fever due to increased production or to diminished dissipation of heat?" There are three ways in which this question has been studied. First, by *a priori* reasoning. This always ended in leaving it an open question. Second, calculations have been based on the amount of food- and tissue-waste. But the third (the experimental method) is the most convincing, and leads to the most satisfactory conclusions.

The experimental method has been used mainly by the Germans. Leibermeister's experiments consisted in subjecting healthy individuals and fever patients to cold baths of known quantity and observing their effects. But there are many sources of error in this plan, chief of which is the fact that cold baths

stimulate heat-production. Leyden used a calorimeter which encased a leg of the patient. Senator experimented extensively upon dogs in a state of health and suffering with septicemia. He made his observations mostly during only one hour in the day. It is a well-known fact that the bodily temperature has a rhythmical variation during every twenty-four hours. This rhythm is very probably connected with a corresponding rhythm in heat-production. The observation, to be strictly accurate, should extend over the entire twenty-four hours. Dr. Wood made a series of seven experiments, the observations extending over the greater part of the twenty-four hours of several days in succession. The animal was fed and put into the calorimeter, and observations were made during the food-day. Next observations were made for first hunger-day. After this septicemia was produced by injecting putrid pus or blood. Observations were now made for several days on heat-production, heat-dissipation, elevation of temperature, etc. I will let him give his own commentaries on his experiments. He says, "I think the following conclusions must be considered as demonstrated: In the pyemic fever of dogs the heat-production is usually in excess of the heat-production of fasting-days, but less than that which can be produced by high feeding. Usually the production of animal heat rises in the febrile state with the temperature and with the stage of the fever, but sometimes the heat-production becomes very excessive, although the temperature of the body remains near the normal limit. In studying the production of animal heat in normal dogs it was found that there were evidently two sources of it, a portion of the heat being produced by the immediate destruction of food taken in excess of the needs of the organism, and another portion being the result of chemical movements in the stored materials of the body." It is evident that "there is increased chemical movement in the tissues during pyemic fever in dogs."

The inductive method used by Wood and the Germans gives results that are in strict agreement with the deductive method used by Burdon Sanderson. From his investigations, taken in

conjunction with those of others, Dr. Wood defines fever in the following words: "Fever is a complex nutritive disturbance in which there is an excessive production of such portion of the bodily heat as is derived from chemical movements in the accumulated material of the organism, the overplus being sometimes less, sometimes more than the loss of heat-production resulting from abstinence from food. The degree of bodily temperature depends in greater or less measure upon a disturbance in the natural play between the functions of heat-production and heat-dissipation, and is not an accurate measure of the intensity of the increased chemical movements of the tissues."

In the part of this essay read at the previous meeting of this society I sketched as briefly as is consistent with thoroughness the latest teachings with regard to certain problems preliminary to a rational theory of fever, among which the following may be considered as solved:

1. The essential symptom of fever is abnormal elevation of bodily heat.

2. "Section of the cord is followed by a decided increase in the giving off of animal heat, and the amount of increase is in direct proportion to the nearness of the section to the brain, provided respiration is not interfered with."

3. Section between the medulla and pons is usually followed by a rapid and decided rise of animal temperature.

4. There is a center somewhere in the cerebro-spinal axis, situated probably in the pons, which regulates animal temperature.

5. The influence of the heat-center is inhibitory in its nature.

6. "In the pyemic fever of dogs the heat production is usually in excess of the heat-production of fasting-days, but less than that which can be produced by high feeding. Usually the production of animal heat rises in the febrile state with the temperature and with the stage of the fever, but sometimes the heat-production becomes very excessive, although the temperature of the body remains near the normal limit."

The remaining portion of this essay will be little else than an

extract from the last chapter of Dr. Wood's work on Fever, and I claim no merit for originality either in the matter or manner of presenting the concluding portion of this paper.

After re-stating his definition of fever Dr. Wood goes on to state that "where all parts of the body are simultaneously affected there must be some bond uniting them together, through which is brought about the simultaneous action. There are only two tissues or systems which, uniting together all parts of the body, fuse them, as it were, into one. These are the blood and the nervous systems. Any acute physiological or pathological process, not dependent upon original vice of constitution, affecting the whole protoplasm of the body simultaneously, must have its origin therefore either in the blood or the nervous system."

He next discusses the question, "Is fever hemic or neurotic in origin?" In many fevers we know that there is a poison circulating in the blood, as, for example, in septicemia, and we may speak of this as hemic in its origin. "If the poison, carried by the blood into all parts of the body, acts upon the various tissues every where in such a way as to increase in them tissue change, or if, upon entering the blood, it excites such changes in that fluid as to cause the blood to incite the tissues every where to fever, then that fever may be called, with scientific strictness, hemic. Suppose, however, there were a fever-center in the nervous system, and that irritation of a peripheral nerve were capable of causing fever by affecting that center, such fever would certainly be a neurosis. Granting the existence of a fever-center, there must be poisons capable of acting upon it directly, so as to produce fever. Such fever would certainly be neurotic, although produced through the blood, the vital fluid acting simply as a 'common carrier.'"

The profession still holds that a large number of febrile reactions are due to peripheral irritation, most of which are caused by inflammation. Billroth and others have investigated these inflammatory fevers, and show it to be highly probable that they are due to the absorption of inflammatory products. Billroth showed that fresh pus when injected into the blood is capable of

producing fever. "In wound-fever a sufficient length of time usually elapses between the reception of the wound and the development of the fever for the dissipation of inflammatory products, and there are many cases of severe wounds in which no febrile reaction occurs, and these cases are notably those in which inflammatory products are scanty." To these arguments Billroth adds his failure to produce in dogs distinct immediate fever by peripheral irritation of sensitive or vasomotor nerves. . . . The experiments of Drs. J. Bremer and Chrobak are of greater weight. In these experiments were used dogs in all respects normal, and others in which all nervous connection had been severed between a specified joint and the central nervous system. The investigators found that traumatic fever is developed with difficulty in dogs. They therefore opened and crushed the joints operated on and injected them with irritants, such as tincture of iodine, ammonia, oil of mustard. Under these circumstances they found that fever was developed as soon where all the nerves going to the part had been previously separated as it was in the normal animal."

Wood has repeated these experiments of Bremer and Chrobac, and obtained like results. "Two dogs were used. All the nerves of the leg were divided and the wound allowed to heal. In one instance the femoral sheath was destroyed and the artery tied in two places. On opening the joint some weeks after the operation and pouring strong water of ammonia over the wound no evidence of sensation was elicited. Nevertheless distinctive fever was manifested in the dog with an injured artery in twenty-four hours, and in the dog with the artery tied after the lapse of forty-eight hours, the slow development of the last case probably being due to a slower circulation and absorption, owing to the local impairment of the blood-vessels. A very strong indication of the truth of the views of Billroth is also to be found in the history of anticepticism." Urethral fever was formerly believed to be due to reflex irritation, but Sedellot proved that it is septic in its origin.

"From these facts it would appear that not only is the so-

called sympathetic fever of inflammation really due to blood-poisoning, but as our knowledge grows fevers supposed to be due to peripheral irritations are shown, one by one, to have their origin in toxemia."

"The history of cases of febrile reactions during teeth-cutting and the relief afforded by relieving the tension of the gums, the fugitive fevers seen in childhood as the product of gastro-intestinal irritation, the various trifling febrile reactions of ordinary life, all seem, however, to indicate a cause more trifling than blood-poisoning, and to point to direct peripheral nerve-irritations as provocative of febrile reactions."

From the foregoing we would conclude that irritative fevers probably do exist, but not to the extent that was once believed, "but that almost all serious protracted attacks of fever are due to the absorption into the blood of a poison. Fever due to the introduction of a poison into the blood appears at first sight to be probably produced by an action of the poison upon the general protoplasm. If, however, we take the malarial fevers, the chill, the fever, and the sweating in their regular sequence, and their periodical occurrence most plainly bear testimony to a neurotic origin. When it is further remembered that neuralgia and various local vasomotor and secretory disturbances (such as intermittent pneumonia and intermittent diarrhea) sometimes replace the normal paroxysm, it becomes almost inconceivable that the normal paroxysm can be produced by a general action upon the common protoplasm of the body. Again, in rare instances the malarial paroxysm becomes localized in a certain region of the body, which may exhibit the successive phenomena of '*a chill*,' while the remainder of the organism seems perfectly normal in its functions."

It may be objected here that when speaking of the malarial poison we are dealing with an unknown factor. This is not the case with septicemia, for we know positively that septic fever has its origin in a definite poison which circulates in the blood. In the first part of this essay I cited experiments that showed that the elevation of the bodily heat of septicemia in dogs was mainly

due to *retention* of heat. "Such retention of heat can only be produced through the intervention of the nervous system, no conceivable influence upon the general protoplasm being able to cause contraction of the superficial capillaries, to which this retention must be in a measure due. The fever must therefore in septicemia be neurotic in origin. From all the facts and reasons that have been given, the following proposition seems to be the logical conclusion: Irritative fever, if it exists, is produced by an action on the nervous system. Fever occurring in cases of blood-poisoning is often and probably always the result of a direct or indirect action of the poison upon the central nervous system, and hence is a neurosis."

"What is the relation of the so-called inhibitory heat-center to the febrile state?" Experimentation shows that in fever, as in health, heat-dissipation is greater after section of the cord than before, while heat-production is greatly diminished. This increased heat-dissipation and lessened heat-production, however, is greatly exaggerated in the febrile state. Wood explains this exaggeration upon the hypothesis that there is "paresis of the so-called inhibitory heat-center" in fever. He writes, "When the normal cord is cut, the paralysis of the heat-inhibitory nerve in some measure compensates for the effect of the alteration of the circulation upon the heat-production; but in fever, if there be already inhibitory heat-paralysis, this compensating influence disappears, and consequently the reduction in heat-production is exaggerated."

In the first part of this essay I pointed out that it had been shown that the test of the integrity of the vasomotor system is the rise of the blood-pressure when a sensitive nerve is galvanized. It was also shown that galvanization of a sensitive nerve caused a *fall* in the bodily temperature. Now if there is an inhibitory heat-center it is through this that the depression of temperature upon galvanization of a sensitive nerve comes. Should there be paralysis of the inhibitory heat-center, no amount of stimulation of a sensitive nerve would affect the temperature. Haidenhain's experiments in this direction showed no

effect upon the temperature, but Wood found that the temperature was considerably depressed when a sensitive nerve was galvanized; but the depression was not so great as in the normal state. There is a discrepancy between the results of the two experimenters, which is no doubt due to the different methods of experimenting. Haidenhain used a weak current, while that used by Wood was a very strong one. Wood thinks that the difference may be explained upon the hypothesis that in fever there is only a partial paralysis of the heat-center. The stronger current would arouse the heat-center, while the weak one would have no effect. In confirmation of the theory of partial palsy of the heat-center, Dr. Wood made a few experiments upon rabbits in which pyemia had been produced. The experiments began usually on the last or last two normal days preceding the fever. "In the first experiment the irritation of a sensitive nerve, applied on the normal day for 3½ minutes, reduced the temperature 0.8°; on the fever day, applied for 4 minutes, it reduced it only 0.4°. In the second experiment, normal day No. 1, the current was applied 3.5 minutes, the fall 0.8°; normal day No. 2, current 6.5, fall 1.6°; fever day No. 1, current 6.5 minutes, fall 1°; fever day No. 2, current 6.5 minutes, fall 1.1°. In the last experiment the time of application and the strength of current were the same throughout, and the fall the first normal day was 2°, the second normal day 2.2°, the fever day 1.6°. These experiments certainly indicate that in the fevered rabbit the peripheral irritations have less effect in depressing the temperature than in the normal animal."

It would appear from this that in fever "the so-called inhibitory heat nervous system is not paralyzed, but is less capable than in health of answering promptly and powerfully to suitable stimuli; in other words, it is in a condition of paresis or partial palsy."

I will here quote the concluding paragraph in Dr. Wood's work, which embodies in a few words his theory of fever: "Bearing these facts in mind, the theory of a causation becomes, to my mind at least, very plain. It is simply a state in

which a depressing poison or a depressing peripheral irritation acts upon the nervous system, which regulates the production and dissipation of animal heat—a system composed of diverse parts so accustomed to act in unison continually in health that they become, as it were, one system, and suffer in disease together. Owing to its depressed, benumbed state, the inhibitory center does not exert its normal influence upon the system, and consequently tissue-change goes on at a rate which results in the production of more heat than normal and an abnormal destruction and elimination of the materials of the tissue. At the same time the vasomotor and other heat-dissipation centers are so benumbed that they are not called into action by their normal stimulus (elevation of the general bodily temperature), and do not provide for the throwing off of the animal heat until it becomes so excessive as to call into action, by its excessive stimulation even their depressed forces. Finally, in some cases of sudden and excessive fever, as in one form of the so-called cerebral rheumatism, the enormous and almost instantaneous rise of temperature appears to be due to a complete paralysis of the nervous centers presiding over heat-production and heat-dissipation."

RICHMOND, IND.

INOCULATION FOR PANNUS, WITH A CASE.

BY W. CHEATHAM, M.D.,

Lecturer on Diseases of Eye, Ear, and Throat in University of Louisville; Visiting Eye, Ear, and Throat Physician to Louisville City Hospital, Kentucky Infirmary for Women and Children, and Masonic Widows and Orphans' Home.

The word pannus means a red cloth. In the more severe forms, such as pannus crassus, the resemblance may be easily traced. Pannus in ophthalmology means a vascular keratitis or vascular inflammation of the cornea. We have the pannus crassus, pannus tenuis, traumatic pannus, etc., these divisions de-

pending upon the extent of the inflammation and its causes. Where the vascularity is very slight, or involving only a small portion of the cornea, it may be called pannus tenuis; when the vascularity extends into the layers of the cornea, and the vessels are very numerous, we call it pannus crassus. If the pannus has for its cause inverted cilia, it is termed traumatic pannus.

The most common cause of pannus is granular ophthalmia, where the rough granulations by their continued rasping of the cornea produce the vascular keratitis. It very often occurs in relapsing phlyctenular keratitis, especially when the phlyctenulæ are near the center of the cornea. In entropion, where the lashes brush against the cornea, pannus is often the result. Whatever the cause be, the first indication is to remove it. In many cases after this is done the pannus disappears; but again in many it persists. In these cases many remedies have been advised. Argent. nit., cupri sulph., acid tannic, and poultices, besides operative procedures, are a few only of the correctives recommended. These may give some relief in the milder cases, but where you have a true pannus crassus to deal with I do not believe any thing will yield the result that inoculation with blennorrheal matter will. To illustrate its use and good effect, I cite a case in which I have lately tried it.

Mr. G., aged forty, a farmer from below Paducah. General health not very good. Came to see me September last about his eyes. I found him with one of the worst cases of pannus I had ever seen. The corneas were so thoroughly vascular that the pupils could scarcely be seen. He was unable to see more than to tell daylight from darkness, or white bodies. The palpebral conjunctivas were thickened and callous. After some weeks' treatment of lid they were greatly changed for the better. No treatment to corneas improved them any. He was also on general treatment at the same time.

Finally giving up all hopes of relief from the usual course, I told him frankly there was but one thing else I could advise with any prospect of relief. I stated the case to him plainly, telling him I would inoculate his eyes with pure gonorrheal matter;

stated the pain he would suffer, the chances of ulceration and atrophy of the globe. He did n't appear to give the affair a moment's thought, but placed himself in my hands to do the best I could with him. After some effort I procured some pure gonorrheal matter of Dr. B. of this city. I everted both upper lids and applied it thoroughly to them. In about three days the lids were swollen and edematous, with some discharge from eyes. In a week the disease was well developed. I did not undertake to control it for about ten days. Then I used iced cloths locally, with a collyrium of tannic acid grs. xx to aqua dest. ℥ j, dropped into eyes three or four times a day. In using this I usually direct that the eyes be thoroughly cleansed with cool water first; then the medicine dropped in; in a few minutes the eyes cleansed again, and the application repeated. Between these applications the eyes were thoroughly cleansed every hour or two. This prevented any increase of trouble and appeared to decrease amount of discharge some. After using this for ten days I ordered a collyrium of argent. nit. gr. j to aqua dest. ℥ j to be dropped into eyes every two hours; the iced cloths to be continued. Under this line of treatment discharge almost stopped in a week. I sent him to the country to remain three weeks. He returned a short time ago revealing remarkable improvement. He walked into my office alone. The corneas were very clear. Vision equal to $\frac{10}{200}$. He said he had no difficulty in going alone any where he wished. I should have said while in the country he continued the argent. nit. drops, and will add for the edification of some oculists that there is not now nor will there be any staining of tissues from the use of the nitrate of silver. Five minutes after dropping the medicine in he usually bathed the eyes well in a weak solution of salt water.

Mr. G. is of course highly gratified and greatly surprised. It was hard to convince him at the height of the disease but that both eyes were gone. His sight is still improving, and I believe will continue to do so for some weeks yet. I believe it will be at least double what it was at my last examination.

This mode of treating pannus is given in works on ophthal-

mology as the *dernier ressort*, and is spoken of as very bold. I shall myself in future in such cases, when both eyes are involved, make it my primary resort. I believe in such cases, where the cornea is highly vascular, that such a procedure—under proper care—is entirely admissible and proper. I do not think it bold. With the cornea in such a condition it is not at all likely to ulcerate and break down. The vascularity is such a safeguard that a bad result is nearly impossible.

Of course when but one eye is involved much caution will have to be exercised. It will be better to bandage the opposite eye, especially during sleep. Should the matter get in the good eye, the disease can be easily aborted by dropping into it a solution of nitrate of silver grs. xx or grs. xl to ℥ j of aqua dest., and the use of iced cloths. To illustrate the efficacy of this, I will state that I had a young man from one of the lower counties of this State in the same room with Mr. G. The young fellow was under my charge for soft cataract. For certain reasons it was impossible to isolate Mr. G. Young H. was accustomed (contrary to my directions) to lead Mr. G. to the water-closet. In doing this his eyes were both inoculated. As soon as I discovered it I ordered the argent. nit. grs. xxx to aqua dest. ℥ j to be dropped into both eyes twice a day, and to bathe them immediately afterward in salt water and apply the iced cloths. The trouble was easily aborted.

In conclusion, then, I believe in pannus crassus there is but one remedy, and that is inoculation by blennorrheal matter; that the treatment is almost harmless; and that there is no estimate of the good resulting.

Gonorrheal matter, or the discharge from a case of purulent ophthalmia, or that from a case of ophthalmia neonatorum, can be used. Whatever the source, be sure it is pure. Exercise the proper caution, and I am sure you will not be disappointed.

LOUISVILLE, KY.

FOREIGN CORRESPONDENCE.

My Dear Yandell: LONDON, January 15th.

I have not very much to write about this week, for at this season of the year the goddess Science gives place to a stick-in-the-mud old deity called Father Christmas, whose great aim appears to be to force people to eat and drink more than is good for them.

Parliament will meet very shortly, and it is confidently expected that the coming session will place our medical examination system on a satisfactory and permanent basis. The delays and the discussions of the last ten or twelve years have prepared the way for a reform that was before impracticable. Nineteen bodies competing with each other, and all comfortably and complacently meeting at the General Medical Council and indorsing their own regulations, were conceived of as an arrangement that could not be touched with impunity. Each body held tenaciously to the one-sided character of its diploma, so long as it admitted to the register and brought shekels to its college. A reputation for passing men with a minimum of knowledge was a most profitable thing for one of these corporations, for it brought to their examinations the lazy brigade and the fools, who wish to sneak into the profession by any back door or trap door that is accessible to them; and such men as these form no inconsiderable percentage of the medical students. Now, however—thanks to the resistance of these corporations to reform of any kind whatever—a royal commission has been appointed to let in light on this costly, clumsy, and indefensible system, and there seems a chance of getting a board in each division of the kingdom which shall be "above suspicion," with guarantees that no minimum shall be devised which will permit different views on the course of the radial artery and the position of the heart, and that when a man passes this board he will have a diploma which will enable him to recover in any division

of the kingdom or in any county-court action charges for any case of medicine, midwifery, or surgery.

The candidates for the pass examination of the College of Surgeons are somewhat anxious, for a new rule is to be put in force in the examination this month. They are to be examined in midwifery, and are to have medical cases to diagnose. Hitherto the examination in medicine at the College of Surgeons has been of a most elementary character, and there has been no examination in midwifery, the student having only to get signed up for having attended lectures and conducted a dozen labors.

There is a great deal of talk over here about the theft of the embalmed body of the late Earl of Crawford and Balcarres from the family vault at Dunecht in Scotland. I suppose you have heard about the outrage. The motive for it, however, is very difficult to fathom. The general belief is that it was stolen in the hope of a large reward being offered for its recovery. If that is the case the "snatchers" have but a sorry chance of realizing their expectations, for the present Lord Crawford is a man of inflexible will, and from the very first he said that he should never offer a reward for the recovery of the body. It seems improbable that the theft was performed by common pilferers, for silver ornaments of some value that adorned the outer coffin were left intact.

Mr. D'Oyley Carte, the manager of the new Savoy Theater, has set an example which, if followed by other *impresarios*, will in all probability prevent the frequent occurrence of fires in theaters, leading to such terrible catastrophes as those of Nice and Vienna. The whole of his theater, both on the stage and in the auditorium, is illuminated by the electric light. The safety of the process by which the Savoy is lighted lies in the fact that the incandescent light is used. This was demonstrated a few nights since between the acts by the following test: A light was enveloped in muslin, and the glass surrounding it then broken, the result being that the light was instantly extinguished and the muslin not even singed. By the substitution of this light the comfort of the audience is materially increased, the quiver-

ing of the heated air is abolished, and the heat of the stage greatly decreased. The effect of this light upon the eye is very similar to that of gas, for it does not possess the dazzling whiteness of the electric arc. The absence of the flickering over the footlights and the sense of security from fire amply compensate the extra expense of this method of illumination. Mr. Carte was so well pleased with the success of his experiment that he presented the actresses of the company each with a gold locket and the actors with a handsome ebony walking-stick, bearing an inscription taken from the parts which they severally played. We wish the manager of the Savoy every success for his spirited undertaking.

Several of the London hospitals have this Christmas had amateur performances of theatricals and concerts for the amusement of the patients compelled to spend Christmas within their walls. This was notably the case at the Seamen's Hospital at Greenwich, where we last week witnessed the performance of a nautical pantomime, written by one of the officers and acted by the resident staff of the hospital, which was deserving of the highest praise. The sea-songs and hornpipes brought down thunders of applause from the audience, which was composed of sailors of various nationalities.

About two years since Professor John Clay, obstetric surgeon to the Queen's Hospital, Birmingham, published a paper in the Lancet on the use of Chian turpentine in the treatment of uterine cancer. The paper attracted considerable attention from both the profession and the public. During the past two years repeated trials of the drug have been made by members of the profession, both in London and the provinces, but have not met with the success claimed for the drug by the professor. Dr. Clay has now written again in the Lancet a long article on this subject, from which the following is an extract: "Now the facts within my own knowledge, derived from my own professional experience, may be summed up as follows: Nine cases of cancer confined to the uterus, which have been under treatment for about twelve months, are so far convalescent that they are no

longer under observation. The cancerous growths have disappeared, there is no bleeding on manipulation, and the parts are smooth to the touch and appear to be covered with mucous membrane. In most of the cases the cervix uteri is shortened from the contraction consequent on removal of the growth. A number of cases of uterine cancer in private and hospital practice are under treatment, in which freedom from pain, diminution of hemorrhage, and sloughing of the growth, with improvement of the general health, are prominent features. . . . In treating a case of cancer of the uterus or rectum the following procedure is recommended for adoption. It may be premised that the genuine drug only should be administered. It is a humiliating statement to make that even now some houses are supplying and dispensing other turpentines for Chian turpentine, and are guaranteeing the genuineness of the article which they are supplying. The vagina and rectum should be syringed daily with equal parts of vinegar and water. After allowing time for this to drain from the parts, it is advisable to insufflate into the vagina or rectum about ten grains of the following powder: tannic acid, half an ounce; powdered charcoal, two drams; and powdered sulphate of copper, ten grains. The propriety of excising the os uteri in epithelioma of this part as a preliminary procedure to the use of Chian turpentine may be questioned on reasonable grounds. The cancerous growth as it disappears under the Chian turpentine treatment leaves a shortening of the os uteri, which brings the external rim of the os uteri into close proximity with the bladder and rectum, so that if the vaginal portion of the uterus has been removed the subsequent contraction of the lower portion of the uterus drags upon the rectum and bladder, causing great pain with rectal and especially vesical troubles. In large pedunculated epithelioma of the os uteri the larger portion of it is perhaps best removed, taking care to leave the normal uterine tissue intact. In cases where the turpentine has been taken for some months the dull curette may be used to remove the sloughing mass with advantage. When pain in the sacral or hypogastric regions comes on after the treatment

has been pursued for two or three months, the use of morphia suppositories is indicated." We can only hope that on further trial the profession in general may succeed in obtaining more of the successful results claimed for the drug in the amelioration of symptoms, if not in the cure of this terrible disease.

The medical officers of the German army who attended the International Congress in August last have just sent over three large groups of their photographs in handsomely-mounted, carved wood frames, the photographs being arranged round an illuminated inscription in the center, for presentation—two of them to Fleet-surgeon W. H. Floyd, R.N., and Surgeon A. B. R. Myers, Coldstream Guards, secretaries of the Military Medical Section of the Congress, and the third to the mess of the Army Medical Department at Aldershot. The photographs, sixteen in number, are excellent likenesses, and the faces must be familiar to those who attended the meetings of the Congress. The German officers have also sent a letter to each of the recipients of the photographs, expressing in the warmest terms their thanks for the attention shown them in London, and regretting that they could not express their gratitude to every one who had shown them so much kindness in this country.

Reviews.

Indigestion and Biliousness. By J. Milner Fothergill, M.D. New York: Wm. Wood & Co. 1881. 8vo. Pp. 316.

"What is one man's meat is another man's poison." So the author thinks, and says that "physiology—an acquaintance with function in health—alone can guide us to a knowledge of disordered function"; and so on the basis of a physiological standpoint the book is written.

The first chapters are devoted to natural digestion, artificial digestive ferments, suitable forms of food, tissue-nutrition, diet and drink, primary indigestion, secondary indigestion (neurosal, reflex, cardiac, and toxemic). Under the head of suitable forms of food the writer describes how to make various preparations of peptonized food by the aid of liquor pancreaticus, as peptonized milk, peptonized gruel, peptonized milk gruel, peptonized soups, jellies, and blanc manges, and peptonized beef tea. Peptonized milk gruel is the preparation with which Dr. Fothergill has obtained the most satisfactory results. Of it he says, "It may be regarded as an artificially-digested bread and milk, and as forming by itself a complete and highly-nutritious food for weak digestions." The manner of making it is as follows: "First, a good thick gruel is prepared from any of the farinaceous articles, as wheaten flour, oat meal, arrowroot, sago, pearl barley, etc. The gruel while still boiling hot is added to an equal quantity of cold milk. The mixture will have a temperature of about 125° F. To each pint of this mixture two or three teaspoonfuls of liquor pancreaticus and twenty grains of bicarbonate of soda (half a small teaspoonful) are added. It is then kept warm in a covered jug under a 'cosey' for a couple of hours, and then boiled for a few minutes and strained. The bitterness

of the digested milk is almost completely covered in the peptonized milk gruel, and invalids take this compound, if not with relish, without the least objection."

The general tenor of the chapter on "the functions of the liver" implies that the disorders and disturbances often empirically referred to it are to a certain extent true, and can now be confirmed by scientific illumination.

Eighty pages are devoted to the phenomena of liver disturbances—biliousness. In order to comprehend the treatment that follows, these eighty pages must be fully understood. The treatment recommended is divided into medicinal and dietetic. Gout and diabetes are caused by liver disturbances, says Dr. F., and it behooves the medical man to keep well in view the future, as well as to recognize the present necessities in cases of liver derangement.

The good old-fashioned blue pill and calomel and colocynth are recommended in derangements of the liver due to congestion, provided there are no complications to guard against. In acute biliousness in children, from excessive feeding, an emetic of ipecac may be all that is required.

There are cases where mercury should be avoided or given with great caution, as when there is albumen in the urine. An apology is made by the author because the use of a non-nitrogenized dietary is so often emphasized in the treatment of biliousness. In an appendix twenty pages are devoted to "the failure of the digestive organs at the present time."

The book is recommended to the profession that they may find out what "biliousness" is and how to treat it. A. M.

Clinic of the Month.

ABSTRACTS OF PROCEEDINGS OF THE INTERNATIONAL MEDICAL CONGRESS.

HOW TO LIMIT AND MORE OR LESS PRACTICALLY NULLIFY THE FORCE OF CONTAGION AND INFECTION BY HOLDING THE BREATH WHILE IN IMMEDIATE CONTIGUITY WITH THE PATIENT—Henry MacCormac, M.D.

Diseases termed infectious, and propagated by miasma from man to man, are communicated, and only communicated, as I believe, by inhalation. Avoid the inhalation of the miasma of disease, and you may prevent the transmission of disease. If we assume the aggregate area of the lining of the lung air-cells to amount to forty square feet, we see at once what an enormous avenue to the admission of the germs of disease is thus furnished when brought into such close proximity with the circulating fluid. If we arrest for the moment the act of respiration we may also in so far arrest the transmission of disease. If the proposition I set forth, namely, that infectious disease is communicated by inhalation, be proved correct, a greatly-increased measure of safety for all who have to deal with such—that is to say, for physicians, nurses, relatives, and, in a sense, at one time or other, the entire community—is obtainable.

The poison of infection, when adequately diluted, does not by inhalation communicate disease. Take, for example, a case of plague or typhus, or Asiatic cholera, or scarlet fever, or diphtheria. In the immediate proximity of the patient the poison is highly concentrated and highly communicable. At a certain distance, so that the poison shall be adequately diluted by admixture with pure fresh air, it ceases to be communicable. By sufficient ventilation the poison of infectious disease is rendered, in fact, incommunicable. In long experience I never saw the poison of fever, when I had the control of the ventilation, transmitted from the sufferer to another person. I have seen typhus fever actually extend to nine persons in one household when ventilation or air-renewal was not attended to. Dr. Jackson, when dealing with the plague in Morocco, prescribed for the patients with

entire personal immunity from an open window elevated but a few feet above them.

I have myself had much to do with Asiatic cholera, and always held my breath when in immediate proximity with the sufferers. I never experienced the malady, though every one associated with me, nurses and medical men alike, suffered more or less. For years I had charge of a fever hospital, and was never attacked by typhus fever, though all the doctors, whether predecessors, associates, or successors, had it. The hospital-nurses, without exception, suffered. In private practice I enjoyed the same security, and I have attended typhus fever in the severest forms and in the vilest and worst-ventilated abodes.

A French physician, M. le Dr. Laval, médicin major des hopitaux, it is stated in the *Moniteur de l'Armée*, August, 1874, finding himself on leave in the regency of Tripoli, and hearing that the plague was raging at Merdj, about twenty hours' journey from Bengazi, repaired thither, and without medical coöperation devoted himself without remission to the aid of the terrified population, preventing the malady indeed from spreading, but, to the infinite regret of every one, falling at last a victim to his own superb humanity. This admirable person I believe would not have perished, and multitudes of others might have escaped likewise, had he and they, as I did, held in the breath, taking in a good chestful beforehand, while immediately contiguous to the sick. I am anxious not to exaggerate the value of the precaution whose adoption I urge. I have waited thus long in order, if I could, only to realize fresh assurance.

On the Prevention of Syphilis—Dr. Cunha Bellem, Lisbon.

1. The propagation of syphilis must be met by the propagation of the principles of morality and of the conscience; the promptings of lust, by the most careful and vigorous hygienic counsels; but the tyranny of syphilis can only be counteracted by the tyranny of inspection.

2. If every one can demand as a right that even as the result of imprudence he shall not contract syphilis, then no one can have the right to transmit it to others.

3. Therefore no one should be free to contract this malady or to maintain it in his own person, for every one should be free from the risk of contagion.

4. In order to guarantee this liberty there is no other resource than the inspection of all persons suspected of having syphilis.

5. By acting on these principles syphilis will in the end be got rid of, or it will at least be banished into the sanctuary of married life,

where it can not be subjected to any controlling influence, but from whence feelings of morality alone will suffice to eradicate it.

THE INFLUENCE OF MILK IN SPREADING ZYMOTIC DISEASE—Ernest Hart, Esq.

The author submits an abstract giving in a tabular form particulars of seventy-one recent epidemics due to infected milk that have been recognized and made the subject of detailed observations in this country, sixty-seven of them since the Marylebone milk typhoid epidemic, traced and reported by Dr. Murchison and the author, in 1873.

The three diseases which have as yet been recognized as capable of being spread by milk are typhoid fever, scarlatina, and diphtheria. There is nothing in the analogy of epidemics to limit the list permanently to these, and already there are indications of other cognate diseases being spread by the same agency. The number of epidemics of typhoid fever recorded in the abstract as due to milk is fifty, of scarlatina fourteen, and of diphtheria seven. The total number of cases traced to the drinking of infected milk occurring during the epidemics may be reckoned in round numbers as thirty-five hundred of typhoid fever, eight hundred of scarlatina, and five hundred of diphtheria. As regards typhoid fever, the most common way in which the poison has been observed in these epidemics to reach the milk is by the soakage of the specific matter of typhoid excrements into the well-water used for washing the milk-cans and for other dairy purposes, and often, it is to be feared, for the dilution of the milk itself, for which, in official reports, "washing the milk-cans" has become a convenient euphemism, advisedly employed to avoid raising unpleasant questions.

In twenty-two of the fifty epidemics of typhoid fever recorded this is distinctly stated by the reporters to be the case, and in other cases it was more or less probable.

When a dairy is unwholesomely or carelessly kept there is obviously a great variety of ways in which the poison may reach the milk. Numerous instances of this kind are given.

Scarlatina being almost invariably spread by contagion and by the inhalation of the bran-like dust which is thrown off from the body during the disease, we should expect in epidemics of milk-scarlatina to receive evidence of this dust having access to the milk; and in the majority of recorded epidemics it was found that persons employed about the dairy operations were in attendance at the same time on persons sick of scarlatina.

In none of the seven recognized outbreaks of *diphtheria* due to milk has it yet been possible to trace the exciting cause of the out-

break, though as to the disease being spread by milk there could be no doubt whatever. It has indeed been suggested whether a disease of the udder of the cow called "garget" may not so affect the secretion of milk as to give rise to diphtheria in the human subject. So far this notion is a mere conjecture unsupported by fact.

The great majority of the cases give statistical as well as experimental support to the conclusion that the responsibility of the epidemic lay with the milk.

It is upon the largest drinkers of the milk (those, namely, who, consuming the greatest quantities, have a correspondingly greater chance of imbibing disease-germs) that the incidence of the disease chiefly falls.

Thus young children (ordinarily little liable to attacks of typhoid), who are accustomed to drink milk largely in the raw state; domestic servants, who, after children, drink the most raw milk; and large milk-drinkers of every rank and station furnish by far the largest quota of cases in each epidemic.

People too who drink exceptionally of the implicated milk are attacked, although the milk taken at their own houses is derived from other sources.

The houses invaded during the epidemics are found to be commonly of the better class and in healthy situations. The poor, who take very little milk, and that only in tea or coffee, commonly escape the disease.

The striking fashion in which the disease "picks out" the streets supplied by the implicated dairy, and the houses in those streets receiving the milk, is noteworthy. People in adjacent houses, and who drink milk supplied by different retailers, escape; and when supplies from two sources enter the same house the disease only attacks those drinking that from the infected source. The contemporaneous invasion of so many households at once can only be explained on the hypothesis of a common cause acting on a particular set of persons and on no others.

ON THE ACTION AND USES OF ANTIPYRETIC MEDICINES, INCLUDING THE INFLUENCE EXERTED BY MEDICINES ADMINISTERED INTERNALLY UPON SEPTICEMIA AND ALLIED CONDITIONS—Prof. Binz.

1. In the present state of our knowledge there are two modes in which antipyretic remedies may be conceived to operate: First, by increasing the discharge of the pyrexial heat; secondly, by checking its production.

2. The quantity of heat discharged may be augmented by direct

withdrawal (tepid water) or by facilitating the circulation through the skin (digitalis, cutaneous irritants).

3. Heat-production may be lessened by repeated cooling of the surface, and especially by the internal use of antizymotics.

4. Febrile diseases commonly owe their origin to the introduction and rapid development of substances akin to ferments. Several of these have been shown to resemble yeast in being low vegetable organisms or derived from such organisms. They enter the glands, where they undergo multiplication, increase the metabolic processes, generate products of decomposition which exert a paralyzing action on the nervous system, and raise the standard of temperature throughout the body.

5. Owing to impaired action of the heart in certain stages of the disorder, or to contraction of the cutaneous vessels, the skin becomes anemic, and gives off less heat than usual. The internal temperature rises accordingly.

6. Quinine, our chief antipyretic, acts by directly combating the efficient cause of the disorder and by checking the abnormal metabolism going on in the body. The nervous system takes no part, or only a secondary part, in this operation. In intermittent fevers quinine prevents the paroxysms by attacking their infective cause. The paroxysms are not the essence, the substantive element of the disease; they are only a symptom of it. The substantive element is the poison deposited in the colorless corpuscles of many organs, especially the spleen. There are fevers without paroxysms, and paroxysms without fever. It is just those intermittent fevers which run their course without paroxysms that are the most malignant. The malarial poison rapidly causes disintegration of the tissues and the blood, and so paralyzes the nerve-centers.

7. The reduction of acute splenic tumors by quinine depends upon the adverse influence exerted by the alkaloid on the infective poison to which the morbid over-action of the spleen and its consequent enlargement are due. "*Cessante causâ cessat effectus.*" Even a healthy spleen may be reduced in size by large doses of quinine, the alkaloid vigorously checking the oxidation of its principal elements, the colorless corpuscles. Quinine has no direct influence on the vasomotor nerves.

8. Quinine attacks the malarial poison with especial energy. On this fact depends the so-called specific action of quinine in intermittent fevers. The same relation, but in a minor degree, subsists between quinine and the infective poison of enteric fever, between mercury and iodine and the poison of syphilis, between salicylic acid and the "irritant" in acute articular rheumatism.

9. An antipyretic which in one disease instantaneously arrests the fever may be wholly powerless in another. The difference depends on the fact that the various antizymotics act very unequally on the individual *schizomycetes* and ferments. One will paralyze them rapidly; by another they will hardly be affected.

10. The past history of therapeutics and recent achievements in the domain of etiology and pharmacology entitle us to assume that by persistent scientific inquiry and practical observation we may succeed in discovering a specific antidote for every species of infective or septicemic malady.

ON THE ACTION AND USES OF ANTIPYRETIC MEDICINES, INCLUDING THE INFLUENCE EXERTED BY MEDICINES ADMINISTERED INTERNALLY UPON SEPTICEMIC AND ALLIED CONDITIONS—Prof. Fokker, Groningen.

While there is no great difficulty in understanding the mode of action of simple refrigeration in the treatment of pyrexia, that of antipyretic remedies administered internally is still obscure. We must assume either that they lower the temperature of the body by interfering with the circulation or that they exert a destructive action, in virtue of their antiseptic properties, on the humble organisms to which the pyrexial phenomena are presumably due. The second of these hypotheses is the more likely one of the two. It may of course be objected that such remedies can never be administered in sufficient quantity to insure their presence in the blood in such proportions as to render it aseptic or at any rate to exercise an antiseptic influence. But it must not be forgotten that the organisms in question have to maintain a "struggle for life" in the interior of the body; so that any hostile factor, though unable of itself to check their multiplication, may succeed in doing so when combined with others equally hostile to bacterial life. It is quite possible, moreover, that antipyretic medicines may accumulate in particular organs, which may then exert a disinfectant influence upon the blood.

Antipyretic remedies may legitimately be given in febrile maladies when the heat of the body is such as directly to threaten the patient's life or even the integrity of his tissues. Under such circumstances those aromatic remedies which are at the same time bacterial poisons should be preferred to physical methods of refrigeration. But when the temperature of the body does not rise to a dangerous height the employment of such remedies in antipyretic doses is undesirable, since we have experimental evidence to show that a degree of heat only a little above the normal temperature of the body is injurious to the

vitality or the virulence of the pathogenic organisms. It is quite possible indeed that the febrile heat may be one way in which the system reacts against these organisms and tends toward recovery. In all cases therefore when the temperature does not rise so high as to be of itself a source of danger, physical refrigeration should be avoided, and the antipyretic remedies, whose twofold mode of action has been alluded to above, should only be prescribed in relatively small doses, such as are inadequate to reduce the heat of the body.

On the Nature and Limits of Physiological Antagonism—Dr. H. C. Wood, Philadelphia.

The teachings of modern science show that all functional activity is the result of molecular movements in the living protoplasm. Secretion can be altered, stopped, or increased by an agent put into the body. As the result of change, arrest, or acceleration of these molecular movements, all remedies acting upon a part must either chemically unite with the material of the part, or else, by virtue of its molecular constitution, increase, diminish, or alter the character of the molecular movements; hence stimulants, depressants, alteratives among dynamic remedies. That there are substances capable of uniting with living material is proved by Gamgee's Researches on Amyl Nitrite. Recent researches have shown that chemical antagonism, so to speak, may manifest itself between remedies within the body (Gamgee's Researches—Amyl Nitrite, Carbolic Acid, and Sulphuric Acid in the Body).

We might expect to find forces antagonistic in their action on the organism, as we find them every where in nature. Singularly enough, most substances which begin by quickening molecular action, when present in excess, arrest it (action of ammonia on the heart); but the rule is not universal.

That there are antagonistic medicines was proved in the report of Professor Prevost at the last International Congress. He also pointed out the difference between antidotism and antagonism. A physiological antidote puts aside or relieves symptoms that cause death (woorara and strychnia). A physiological antagonist acts in direct opposition to some other substance (chloral and strychnia). Antidotism may or may not be reciprocal; antagonism is always so. Reasons for this. Many instances of supposed antagonism have really been instances of antidotism. A medicine may be antidotal to itself, though no one would call it antagonistic (illustrated by action of veratroidia, from *veratrum viride*, upon the heart).

The relations of experimental pathology and experimental therapeutics are so close that it is impossible to study the one without the other. Hence the importance of investigations on the antagonism of remedies. An antagonistic treatment of disease is as much within the bounds of possibility as an antagonistic treatment of poisoning. In most cases, however, the action of a natural or "disease" poison is so complex and so little known that we are thrown back on antidotal treatment—that is, the treatment of symptoms—knowing that if we can only keep the patient alive long enough he will recover by the elimination or destruction of this poison, just as he would recover from opium-poisoning if life could be maintained by artificial respiration.

If the distinction between antagonism and antidotism be clearly borne in mind, the mixed falsity and truth of *similia similibus curantur* is seen. Take the action of veratroidia upon the heart. In large doses it paralyzes, in small doses it stimulates the pneumogastric. Supposing the pneumogastrics to be depressed, and the heart's action consequently too rapid, veratroidia in minute doses might be useful. Supposing, on the other hand, that the heart has been paralyzed by an excessive dose of veratroidia or some similarly stimulant drug, could it be expected that minute doses of veratroidia would restore the action of the heart?

It is plain that while allopathy—the doctrine that a symptom must be met by a remedy which produces an opposite symptom—is no more true than homeopathy, the law of antagonism is of wide applicability in therapeutics, and that its range must increase continually with every increase in our knowledge.

On the Remedies used to Promote Absorption of Inflammatory and other Morbid Products—Dr. Dujardin Beaumetz.

Rightly to estimate the value of therapeutic interference under the circumstances indicated above, we must first of all consider the changes that normally occur without it.

The living cell is the theater of a continual integration and disintegration, the materials for which are supplied by the blood and lymph. Under irritation it only undergoes visible alterations, often in the direction of inordinate development. After a time, however, this pathological activity usually comes to a stop, and the abnormal elements which have been produced may gradually disappear. Their disappearance may be brought about, first, by the removal of corpuscular elements, still in an early stage of organization, by the lymphatic circulation; secondly, by fatty or granular degeneration of the newly-formed ele-

ments and the subsequent absorption of the *detritus* thus produced; lastly, by fibroid elements taking the place of the embryonic tissue and choking its further development.

Sometimes in the case of serous and mucous membranes we find sero-fibrinous effusions poured out, which are reabsorbed by the capillaries, either unchanged or after undergoing fatty-granular metamorphosis.

Lastly, the influence of the nervous system in controlling tissue-metabolism must not be left out of account, though its *modus operandi* is still obscure.

Therapeutic interference may, to a certain extent, promote the absorption of these morbid and inflammatory products. Medicines designed to favor it have been termed *resolvent.* The old pharmacopeia divided them into *solvent, deobstruent,* and *absorbent* remedies; the first intended to soften the morbid products, the second to facilitate the circulation through the capillary and lymphatic vessels, the third to promote the absorption of the products after they had undergone more or less profound changes.

This old classification furnishes a very real explanation of the phenomena. To hasten absorption we need only assist the processes, which, as we have already seen, normally occur in the diseased tissues. We try to hasten their return to their embryonic phase, or their fatty-granular degeneration, or the development of the fibroid cicatricial tissue which tends to strangle them.

The remedial measures at our disposal for the promotion of these objects are manifold. Some act mechanically (compression, massage, etc.); others, of a more active kind, by evulsion (mode of action of blisters, "solvent" ointments, and plasters).

Other remedial agents act still more directly on nutrition, through the nervous or the vasomotor system (the continuous galvanic current, applied to the surface, or by electro-puncture). A destructive action on the morbid products, designed to promote their degeneration and absorption, has been aimed at in other ways (injection of *carica papaya* or *papayin* into malignant tumors, toxic injections to destroy the fetus in extra-uterine pregnancy, etc.).

Again, we may sometimes be able to evacuate the liquid products of inflammation by surgical means (pus, sero-fibrinous effusions, etc.).

There remain to be considered those medicaments in the strict sense of the term which have a solvent action on inflammatory and other morbid products. First, however, the influence of the general nutrition of the body on the nutrition of such product deserves notice.

Both augmentation and diminution of nutritive interchange exert such an influence. The former may cause morbid products to be absorbed by stimulating the functions of the economy (for example, removal of strumous and lymphatic deposits by appropriate hygienic measures—country air, nourishing food, regulated exercises—these measures increasing the activity of the capillary and lymphatic circulation). Abstinence and diseases productive of malnutrition may achieve the same end in another fashion (autophagy of muscular and adipose tissue, similar action on tumors chiefly made up of fat). A like method has been proposed with a view to the removal of inflammatory exudations into serous cavities (peritoneum, pleura).

Apart from medicines which act on certain inflammatory exudations (for example, diuretics and purgatives in pleuritic and peritoneal effusions), and from such as operate by affecting nutrition (for example, arsenic), we have only two drugs which exert a real and selective action on the nutrition of such new formations—iodine (including iodides) and mercury. Clinical experience affords daily proof of their value; experimental research has not yet explained it (critical examination of the various hypotheses that have been put forth with this object). We only know that these remedies have a selective power over certain products, and not over others which may appear to be structurally identical with them. Thus syphilitic gummata, strumous deposits, and tubercles are histologically similar; but mercury only affects the first, iodine the second, while the progress of the third is in no degree influenced by either of the two medicaments.

Hemlock and its preparations were formerly in great repute as solvents. Notwithstanding the discovery of conia, however, we are still without any explanation of this special action of hemlock. It may possibly be due to its influence upon the nervous system. Its true value in this respect deserves further clinical investigation.

On Mercury as an Antiphlogistic and Antisyphilitic—Dr. Moinet, Edinburgh.

I uphold the action of mercury as an antiphlogistic in inflammation of serous membranes, not in virtue of any action it has on the blood, defibrinating or otherwise, it never being necessary or advisable to push its action so as to affect the blood to secure its antiphlogistic effect, which I believe is due to its increasing the functional activity of the glandular organs, and thus causing a certain amount of derivation of blood from these membranes sufficient to diminish the inflammatory process.

In syphilis mercury can not be considered either a chemical or physiological antidote for the venereal poison, nor yet curative by an action on the blood. The true position for the employment of mercury in syphilis, the only one warranted by its known physiological action, is that of a palliative for certain of its symptoms, especially troublesome secondary eruptions. In this respect its action I consider is purely a local one, however the drug is administered. In other words, to be brief, it is not a remedy for syphilis as such, but only a palliative for certain of its symptoms. The only treatment of syphilis is that by general hygienic rules, and relief of the symptoms as they appear, for which latter purpose mercury is sometimes useful. But not, I repeat, is there any evidence by its constitutional action, but purely by its local influence on the skin. Its constitutional effect would only aggravate the syphilitic cachexia by depressing the general health of the patient. It is therefore, strictly speaking, not a remedy for syphilis, but one of our means for treating its manifestations.

On Bromide of Ethyl—W. Squire, M.D., F.R.C.P.

Bromide of ethyl was obtained by Sérullas in 1829, and called bromhydric ether. It has the formula C_2H_5Br., that of a direct combination of bromine with the radical ethyl. It closely resembles iodide of ethyl, and is nearly like chloroform. Nitrous ether ($C_2H_5+NO_2$) or nitrite of ethyl, nitrite of amyl, and even nitroglycerin are also allied. The chief therapeutic uses of this bromide are (1) as a local anesthetic, (2) as a general anesthetic, (3) in smaller doses as a vasomotor stimulant or sedative. First, as to local anesthesia, like ether it boils at 105°, and by rapid evaporation will freeze a limited part of surface, and so prevent the pain of incision; but with this the insensibility to pain is produced before freezing begins, and the vapor has an advantage over ether in being much less inflammable. Applied as a compress it diminishes sensibility. Long contact will set up local irritation and even vesication after anesthesia. The pain of deep-seated neuralgia is relieved before the local irritation is produced. As a general anesthetic the bromide of ethyl comes very near the chlorides of methyl in efficacy, and is safer. It neither depresses the heart as chloroform does, nor raises the blood-pressure as does ether. There is no cough excited and no mucous secretion. First used by Thomas Nunneley, of Leeds, in 1849, and strongly recommended by him in 1865, it has recently been brought into notice by Dr. Turnbull, of Philadelphia, and much used by Dr. Levis there. Two drams of it will generally produce complete anesthesia in from three to five minutes.

Recovery is sometimes as quick as after nitrous oxide. I have given half an ounce to a girl eight years old, maintaining incomplete unconsciousness for some time, without any ill effects. Dr. Levis has kept up complete anesthesia for forty minutes with eleven drams of it. In one case of Battey's operation, lasting one hour and a half, nearly five ounces were given. The patient died next day. Unlike chloroform, it will readily burn, and undergoes change in the body. A strong smell of bromine may be noticed for a day or two after large doses. Some of its therapeutic uses may be due to this, though it may present an obstacle to its free use as an anesthetic. In the medicinal use of bromide of ethyl by inhalation unconsciousness need not be sought. Twenty to thirty drops will saturate a square inch of lint. This held to the open mouth on a handkerchief during three or four deep inspirations will cause a tingling sensation down the arms, with a little feeling of fullness about the head and ears. The breathing is free and deeper. The pulse, a little fuller and softer, is not quickened. These slight effects disappear in a few minutes, when, should the inhalation have only partially relieved the dyspnea, megrim, or neuralgia for which it is used, it is to be repeated. The condition requiring relief returns then less rapidly, after a longer and longer interval, or not at all. In this way I have seen headaches of long duration, beginning with tight pain across the temples, and ending in violent throbbing, arrested and prevented; intense side-stitch, suggestive of cardiac disease, recovered altogether; dyspnea, both vascular and renal, effectually relieved; and spasmodic cough controlled. This remedy may even be useful for diagnostic purposes, as it affords no relief to the symptoms of intra-cranial pressure or hyperemia, nor to the referred pains of subacute or chronic visceral disease. It is most useful in neuroses accompanied by vasomotor disturbance, with alternate spasm and relaxation of vessels, and sometimes both states present at different parts of the same vessel. For the various symptoms of marked hysteria it is less useful. In hysteria and epilepsy a full trial has been made at the Bicêtre by MM. Bourneville and D'Olier, ten patients having had more than five hundred inhalations during two months. Some of their results will be given in this paper.

On Pilocarpin—William Squire, M.D., F.R.C.P.

Pilocarpin is the alkaloid obtained from the leaves of jaborandi, on which the efficacy of that drug, long known in Brazil, in producing sweating and salivation, depends. Another alkaloid of different and even antagonistic properties, named jaborin by Harnack and Meyer,

coexists with it, and renders the infusion or tincture of jaborandi less certain and perhaps less safe than that of the pure alkaloid. It is possible that pilocarpin itself has not always been obtained quite free from admixture with its associated but antagonistic jaborin. Muriate of pilocarpin in simple solution is the best form to use—one grain to fifteen minims of water for hypodermic injection. One grain to four ounces of water for internal use are convenient proportions. One third grain is the largest, one fifteenth grain the smallest dose needed. My plan is to give a full dose at once. Others give small doses every hour with some warm drink or alcoholic stimulant till perspiration and salivation are freely established. A dram of the tincture, made with thirty grains of leaf, is equivalent to one third of a grain of pilocarpin. One fourth of a grain of the muriate injected hypodermically will in a few minutes produce suffusion of the face, quickened pulse, some throbbing in the neck, and a general feeling of warmth, followed by free perspiration. This is soon streaming profusely from all parts of the surface, and continues long after the skin has become pale or even cool. The pulse subsides, while the force of the heart's impulse is rather increased There is a tendency to sleep, and generally a fall of temperature. The perspiration goes on for three or four hours. There is an increased flow of saliva, and some increase of pharyngeal and sometimes of bronchial mucus that may give rise to trouble during sleep and require attention. Such a quantity of saliva may be swallowed as to excite vomiting. No headache, sickness, or depression has been noticed by me as a direct result of this medicine. All the secretions of the body, except the intestinal, are increased by it. The quantity of urine, hardly lessened during perspiration, is increased afterward. I have not met with dysuria. Swelling and tenderness of the submaxillary salivary glands has remained for a day or two after profuse ptyalism. The action of the drug is on the peripheral secreting apparatus, and not on the nerve-centers, except so far as the first action on the vasomotors may dilate the vessels and allow the agent freer access to the glands.

Atropia is directly antagonistic to it in this respect. Aconite dilates the vessels, but weakens the heart; while pilocarpin, having no such effect, may even allow of small hemorrhages occurring. It is not anesthetic. The perspiration induced by it does not relieve dysmenorrhea, sciatica, or colic. Pilocarpin does not moderate specific fevers; but given near the time for the separation of the false membranes in diphtheria, it aids the fall of temperature and favors sleep. Where there is already a tendency to collapse, of course it can do no good. It is

useful in the febrile relapse of scarlatinal nephritis. I have met with no confirmation of the observation that small doses (one twentieth of a grain) will check perspiration. A similarly homeopathic view that particular diseases must have particular remedies has led to the statement that pilocarpin is unsuited to renal disease. The use of it has been chiefly in the different kinds of Bright's disease. It may be unsuited to that particular form where dilated vessels and diminished blood-pressure are associated with a large quantity of albumen, yet in these very cases it is serviceable to the intercurrent exacerbations and conditions of accidental congestion not infrequent in their course, and it is preferable to the hot pack or vapor bath. In the early stages of interstitial nephritis of gouty origin it is of great benefit. In the chronic course these cases generally follow it is often useful. It may be resorted to in some of the extreme effects of renal dropsy, and the relief obtained is not accompanied by great depression. In the chronic results of parenchymatous nephritis, as after scarlet fever, it has been found useful; and that it need not be withheld in some cases of scarlet fever itself is proved by the remarkable results obtained from it by Guttmann in the treatment of the allied disease diphtheria.

The Utility of Strychnia as an Expectorant—Dr. Milner Fothergill, London.

The experiments of Prokop Rokitanski have shown that strychnia is a powerful stimulant of the respiratory centers. Without acquaintance with his experiments I arrived independently at the same conclusions from experiments upon rabbits. When the respiratory center was paralyzed by aconite the injection of strychnia exercised a most potent influence in restoring the respiration. Clinical observation corroborates this view, and at the Hospital for Diseases of the Chest I have found strychnia most useful when the respiration was embarrassed. In acute bronchitis, when the act of expectoration is difficult, it is useful. In chronic bronchitis and emphysema it relieves the laboring respiration, and when the right ventricle is dilated adds to the efficacy of digitalis most usefully. In lung consolidation it is also of service. Indeed in all cases where the number of respirations mounts over the ordinary proportion to the beats of the heart (about four to one) it has seemed to me to be of the greatest utility.

INJURIES OF THE BRAIN, WITH GENERAL AND WITH LOCAL SYMPTOMS.—E. v. Bergmann (*Volkm. Klin. Vorträge*) in a clinical lecture condemns the sharp distinction made between cerebral shock and cerebral compression. He states that the general symptoms observed in both these traumatic conditions are referable to a more or less considerable disturbance of nutrition of the whole brain, which, according to the irritability of the various sections of the brain, reveals itself in paralytic or irritative phenomena. The cortex is earliest affected in all cases, the centers situated in the medulla (vasomotor and vagus centers) are implicated later. Slight concussion causes only a transitory confusion resulting from shock to the nerve-elements, or a vasomotor disturbance of the surface of the brain. A more severe one has, as a consequence, more lasting benumbing of the faculties and retardation of the pulse, with irregularity of the respiration from a more pronounced paralysis of the cortex, and with it irritation of the automatic centers in the medulla. A still severer shock produces quickening, weakening, and smallness of the pulse, together with deep coma in consequence of paralysis of the central organs involved. A compression of the brain from extravasation of blood between it and the dura, when slight, may cause also only a moderate, transient benumbing of the faculties, but when more extensive causes more lasting unconsciousness, with sopor and slow pulse, and later, coma with small, rapid pulse. The cortical paralysis which asserts itself variously from mere confusion to the most profound coma is in the first case the result of nutritive disturbances in the nervous elements, accompanied later by vasomotor disorder or capillary hemorrhages in the cortex; in the second case the coma is the result of anemia caused by the increasing pressure having a greater extension over the cortex inhibiting and destroying the function of the nerve-elements. The same cause affects the automatic organs, first causing irritation and then their paralysis. Any distinction between the phenomena of cerebral shock and cerebral compression is only afforded by the order in time and the duration of the symptoms. In cerebral shock the symptoms are of early occur-

rence, and, in favorable cases, early in disappearing. In cerebral compression they increase slowly or rapidly but continuously, and they last longer in favorable cases, even if the extravasation is absorbed. If after injury to the skull the cerebral symptoms are steadily severer, the coma more profound, the respiration stertorous, and the pulse steadily retarded, then increasing pressure is to be diagnosed, caused by an extravasation, and trephining, for the stoppage of the bleeding, is needed. If after rather quick-appearing, transient, severe cerebral symptoms there is left a dullness with confusion and drowsiness, while the pulse and respiration are normal, then the first symptoms are probably due to a cerebral shock accompanying the traumatic injury to the nervous substance, while the later confusion, etc. are due to an extravasation upon the surface of the brain not large enough to cause serious compression, but yet sufficient to disorder the functions of the sensitive brain. If a large extravasation becomes absorbed the disturbances of the pulse and respiration disappear first, the mental confusion last. Von Bergmann found in these cases urobilin in the urine—a result of absorbed coloring matter of the blood. Stasis papilla is not necessarily present with an intracranial extravasation. It is often lacking, and may, moreover, occur (according to Berlin) with fracture of the basis cranii (without extravasation), as when the fissure crosses the optic canal and ruptures the nerve-sheath, blood from the former enters the latter. The brain injuries with local symptoms form a natural counterpart to those with general symptoms. They occur when preferably a more or less circumscribed portion of the brain is injured. In that case the special symptoms connected with the injured part are most prominent. But if at the same time the whole brain is more or less involved, whether as a consequence of shock or through pressure from a rapidly-increasing extravasation of blood, then they only will require consideration together with the general phenomena, whether the latter are slight or retrogressive. Localized brain-symptoms occur especially prominent with lesions of the motor zone, and appear as definite combined paralytic and irritative phenomena

on the opposite half of the body. From these symptoms the locality and extension of the injury in the motor zone can be definitely known, and the case treated accordingly. Broca has given directions for the orientation over the motor region on the skull, and these the author copies. Still another method is given by Lucas Champonniere. Still the author considers both methods, which are given in Lucas Champonniere's monograph on localized trepanation, as nôt altogether satisfactory, and the last one is somewhat complicated. Von Bergmann reports one case in which he successfully trephined a funnel-formed depression of the right temporal bone of some three fourths centimeter circumference. He takes the occasion to recommend, after removal of fragments of bone and careful antiseptic cleansing of the wound, the utmost possible cleanliness of the skin-margin of the wound above the trephined place. The cutaneous wound is closed over the opening, through which a drainage-tube is laid upon the brain. . . .

The author adds to this case instructive remarks upon the phenomena of cerebral edema, which occurred in the vicinity of the wound, and with this connects the paralysis of the left arm that appeared some hours after the operation, disappearing again in a few days, to which were added now and then contractions in the muscles supplied by the left facial nerve. From these symptoms Bergmann thinks that the spot of the cortical injury must be sought for in the anterior margin of the anterior central gyrus, where it borders the third frontal. (Journal of Nervous and Mental Disease.)

Treatment of Subcutaneous Nevi.—Carey Combs reports, in the London Lancet, a new method of treating subcutaneous nevi. A child nine months old was submitted to him for operation upon a nevus three fourths of an inch in diameter upon the left ala nasi. The use of gold needles with the galvanic current not being practicable, two very fine silver wires were passed through so as to run parallel about a fourth of an inch apart, and the ends were connected with a Bunsen's battery. As soon

as the wires became hot the current was broken, the ends of the wires were united to each other, and allowed to remain in the skin with a covering of lint and plaster. One week later the galvanic current was again applied, and three times after that. Then the wires were withdrawn, and the nevus was found to have entirely disappeared.

This operation commends itself above others on account of the trifling pain it causes, its harmlessness, and the ease with which it is performed, insomuch as the insertion of the needles has not to be repeated. Besides this, the fine silver wire that is used requires only a single element of the Grove or Bunsen battery.

Is Anteflexion of the Uterus a Cause of Dysmenorrhea? At a recent meeting of the London Obstetrical Society Dr. Herman read a paper on the above subject. On reading it one may well ask whether there is any thing certain in gynecology, for the author makes an elaborate and well-sustained argument against the prevalent view that anteflexion is a cause of dysmenorrhea.

Few persons educated in New York have not heard the pathology of obstructive dysmenorrhea graphically described, and the neck of the uterus compared to a gutta-percha tube, which is obstructed when bent at an angle. The illustration is simple, and at first thought convincing. The purport of Dr. Herman's argument against this view is as follows: 1. There is no anatomical evidence that anteflexion causes any hindrance to the escape of menstrual fluid. 2. There is reason to think that anteflexion is present in nearly half of all women who have not borne children. 3. Therefore it is to be expected that anteflexion and dysmenorrhea would frequently coincide. 4. Dysmenorrhea is practically as common when the uterus is straight as when it is anteflexed. 5. Painless menstruation is practically as common when the uterus is anteflexed as when it is not. 6. When dysmenorrhea and flexion go together the severity of the pain bears no relation to the degree of the bending.

7. Dysmenorrhea associated with anteflexion is frequently cured without straightening the uterus. 8. There is no evidence that straightening the uterus invariably, or even frequently, removes dysmenorrhea which is associated with anteflexion and in which other methods of cure have been ineffectual. 9. These facts show that the relation between anteflexion and dysmenorrhea is not one of cause and effect, but of coincidence.

The paper of Dr. Herman was highly commended by the president of the society, Dr. J. Matthews Duncan. (The Medical Record.)

ALCOHOL IN THERAPEUTICS.—Lewis D. Mason, M.D., of Fort Hamilton, N. Y., writes, in the Medical Record:

In a course of addresses on Abstinence, delivered before the Hunterian Society of London in 1878, Dr. Benjamin W. Richardson sounded the keynote when he advocated the use of alcohol in medical practice in lieu of beer, wine, whisky, and other liquors containing alcohol. Within the past few years the medical officers of the Inebriates' Home at Fort Hamilton, N. Y., have adopted Dr. Richardson's suggestions in the treatment of the various forms of alcoholism when the use of alcohol was indicated, and have found the following benefits to proceed from their adoption:

First. The satisfaction of having a solution containing a definite percentage of alcohol, thus providing an exact system of dosage.

Second. A very marked saving, and therefore a method of value from an economical point alone, especially in those institutions where the liquor-bill is a large item of expense.

Third. The moral effect on the patient, in compelling him to at once break off his accustomed stimulant and providing a "medicine" as an efficient substitute.

Fourth. The advantage which the alcohol has over brandy, whisky, etc., is that owing to accuracy and concentration of the dosage the patient convalesces in one half the time and with less suffering.

In the annual report for 1880 of the Asylum for the Insane, London, Canada, Dr. R. M. Bucke, superintendent, says: No beer, wine, whisky, or brandy has been used in this asylum during the last twelve months. In place of these, in certain cases of illness where alcohol appeared to be indicated, we have given this in its pure form—mixed of course with water, as other medicines are. In this way we have

consumed in the course of the year four gallons, six pints, and fifteen ounces of alcohol, equal to about nine gallons of whisky, or one gallon of whisky to every one hundred patients under treatment, as against (in former years) three hundred dollars' worth of beer, wine, and whisky to every one hundred patients treated."

Another advantage which this method of prescribing alcohol possesses is the lesson which the community will receive in a quiet way that the proper place for alcohol in any of its forms or combinations is the shelf of the pharmacist.

Moreover, in a properly-compounded prescription containing alcohol the physician will have a much greater control of the future physical and moral welfare of the patient than if he directed him simply to take alcohol as contained in one of the various forms of liquor in common use.

It may be added that Dr. Norman Kerr and other London physicians of eminence have indorsed and adopted the plan of Richardson, and the quotation from the report of the asylum in Canada shows that the method is being adopted in other institutions besides the Inebriates' Home at Fort Hamilton.

We may further note that it is necessary the alcohol should be disguised by the addition of the various carminatives or bitter tonics, and also colored by caramel, so that the patient may not be unnecessarily informed as to what he is taking.

We use what we term our "absolute mixture" in treating patients, as a rule, and without their knowledge as to the fact that they are taking alcohol.

We further find that the ninety-five-per-cent alcohol is equally as efficient as the absolute which we formerly used and which was more expensive.

A Case of Simultaneous Dislocation of Both Hips.—Dr. W. O. Roberts, Adjunct Professor of Surgery, Medical Department of the University of Louisville, reported the following in the Louisville Medical News:

The new edition of Holmes's System of Surgery (Packard) contains the following, which I take to be a full list of all the cases of this accident so far reported:

"Occasionally, but very rarely, both hips are dislocated simultaneously. Hamilton quotes two such cases—one from Gibson and the other from Schinzinger. T. C. Barker reports the case of a boy, aged

nineteen, who had both femora dislocated into the thyroid foramina (the left thrust through into the pelvis) by a fall of thirty feet into a sand-bank. H. L. Prichard records that of a boy, aged fifteen, who was 'doubled up' under a truck and had both femora luxated upward and backward. Boisnot reports a dislocation of the right femur on the pubis, the left upon the dorsum ilii. Mr. Pollard reports a case in which the left hip was dislocated upward and backward, and the right downward and forward into the thyroid foramen. Dr. Crawford, of Wilkesbarre, also reports a case in which the right was displaced upon the dorsum ilii and the left into the ischiatic notch. Allis records a case in which the right femur was displaced on the dorsum ilii and the left into the thyroid foramen. Packard gives an account of a man who was caught under a falling house, and had the left femur dislocated into the thyroid foramen, the right on the dorsum ilii."

I am able to add to the record one other case of this rare injury.

Two years ago J. L., a healthy, well-developed man, aged sixty-five years, while in a stooping position was struck by a falling pile of planks and borne to the ground, the lumber burying him as it fell. The patient was immediately gotten out from among the planks and taken to his home, where, six hours after the accident, I saw him, in company with the late Prof. Cowling. He was in bed, lying propped with pillows, his body inclining to the left side. His left lower limb was abducted, semi-flexed, and measured five inches longer than the right. The right was adducted, semi-flexed, and rotated inward. Over the site of the joint the left was flattened, while the right at this point was full and rounded. With the right limb adduction, flexion, and internal rotation were possible; abduction, external rotation, and extension impossible; while with the left, abduction, flexion, and slight external rotation could be made with ease.

Diagnosis: Head of right femur on dorsum ilii; left, in the thyroid foramen.

The patient was then placed on the floor — a situation which I believe has manifest advantages over any other, not only in the management of hip dislocation, but in shoulder-dislocation as well—and both dislocations were reduced by manipulation after Reid's method. The left limb was the first manipulated, and after reduction it was discovered to be still three inches longer than the right. The right dislocation was next reduced, when measurements showed both limbs to be of equal length.

The function of the left limb was soon restored, but in consequence of injury to the great sciatic nerve a partial paralysis supervened in the

right. Under the persistent use of massage and electricity the patient slowly recovered the use of this limb, walking on crutches for eighteen months, when he discarded these for a cane, by the aid of which he was able to go about and attend to his work with comparative comfort.

The not uncommon condition of false reduction, where under manipulation the head of the femur slips into the great sciatic notch instead of the acetabulum, was excluded in this case by means of Allis's test. The limbs being found of equal length when measured upon a plane longitudinal to the body, were now brought to right angles with the same; and as no disparity of length was shown by this change in position, the completeness of the reduction in both hips was placed beyond question.

A Modification of Lister's Antiseptic Dressing.—In a short paper on his method of dressing wounds, Dr. James L. Little, Professor of Clinical Surgery in the University of the City of New York, writes, in New York Medical Journal:

I have for several years been surgeon to a large factory in this city, in which three thousand hands are employed, and where injuries by machinery are quite frequent. These injuries consist chiefly of wounds of the hands and fingers, caused by their being caught in the cog-wheels and other parts of the machinery. In many cases the fingers are torn off, tendons are pulled from their sheaths, joints are opened, and the hands are often severely crushed and lacerated. In all these cases I have for the past six years been using the following simple antiseptic dressing. Having put the parts in a condition for dressing, I wash the wound in a solution of carbolic acid of the strength of one to twenty. I then cover the parts with a thick layer of borated cotton, and then snugly and evenly apply a simple gauze bandage. At first I used bandages made of antiseptic gauze, but for the past three years have used those of plain uncarbolized cheese-cloth. These thin bandages distribute the pressure more evenly over the cotton, and are more easily saturated with fluids than those made of unbleached muslin.

The patient is instructed to keep the outside of the dressing wet with a solution of carbolic acid of the strength of one to one hundred. I frequently employ Squibb's solution of impure carbolic acid, which is of the strength of one to fifty, and which when mixed with an equal bulk of water gives a solution of the desired strength.

The parts should be kept at rest, and the dressings may be left undisturbed for several days, unless there is pain, rise of temperature, or discharge through the dressings. These conditions are always to be considered indications for redressing.

In many cases where rubber drainage-tubes have been used they may be removed at the second dressing, and if catgut has been used for sutures this second dressing can be allowed to remain on for an indefinite period. In a number of cases of lacerated wounds I have allowed the first dressing to remain on until the wound has entirely healed. In these cases the external use of carbolic lotion was discontinued after the fifth or sixth day, and the dressings would become dry and hard, the wound healing, as it were, "under a scab."

The patient should be instructed to loosen the bandage at once if any pain occurs.

My experience with this dressing covers, as I have said, a period of about six years, during which time I have treated nearly three hundred cases of open wounds. Not one of this number has been followed by inflammatory symptoms. Extensive lacerated wounds have healed and dead tissue has sloughed away without giving rise to any of the so-called symptoms of inflammation. Neither pain, redness, heat, swelling, nor constitutional disturbance has resulted. In no case has there been reddening of the lymphatics or tenderness of the glands. No counter-openings have been necessary. Pain has been entirely absent, so that anodynes have not been needed, save in a single case, and that for one night only, to control slight restlessness.

These results are the more remarkable from the fact that many of these patients were in an unhealthy condition, some suffering from anemia, some from cardiac disease, phthisis, and the like.

The value of cotton wool as an antiseptic dressing is, I think, not fully appreciated by the profession. Used in the way I have indicated, it seems to me to be as perfect an antiseptic dressing as the gauze and other materials recommended by Mr. Lister, while at the same time it is free from all objections that pertain to the latter. If applied in sufficient quantities round an open wound it protects it thoroughly from the "floating matter of the air." It is the best germ-filter known to us. Tyndall found that while filtering the air, and endeavoring to get it perfectly pure, atmospheric dust, which would readily pass through sulphuric acid and a strong solution of caustic potash, was completely stopped by ordinary cotton wool.

I use the very excellent borated cotton made by Ende, of Hoboken, containing fifteen per cent of boracic acid. [A great deal of the

so-called borated cotton sold by dealers is made with a solution of borax, instead of boracic acid, which can always be ascertained by burning a piece of the cotton. If the cotton has been properly prepared with boracic acid the flame is of a bright green color; but if, as is generally the case, borax has been used, the flame will show very little of the green tint.] Keeping it wet externally with the solution of carbolic acid, in the manner already described, renders it more surely antiseptic.

To insure success in the cases where the dressing is used, full precautions as to rendering the instruments, sponges, and the hands of the surgeon aseptic, and the use of drainage-tubes, if necessary, should not be neglected. Catgut or torsion should be used to arrest hemorrhage. The spray may be resorted to, if thought necessary. At the second dressing I now usually apply carbolized oil of the strength of one to twelve to the wound, to facilitate the removal of the cotton, which is otherwise apt to adhere after the first dressing.

The borated cotton is easily kept for months unchanged. The fact that the dressing need not be done oftener than once in several days will especially commend it to the country physician.

The success of this procedure in the treatment of large wounds after accident or amputation will increase its importance and materially extend its field of usefulness.

THERAPEUTICS OF OL. SANT. FLAV.—Dr. R. Park, of Glasgow, Scot., writes, in Practitioner:

It is no use prescribing ol. sant. flav. for the purpose of *curing* a gonorrhea, if by the term is meant the urethritis or other pathological condition causing discharge. The latter, however, is the most troublesome symptom—that which the patient is most anxious to get rid of at once; and for this purpose the ol. sant. flav. is distinctly the most specific drug I am acquainted with. It restrains the "running" at once, very frequently stopping it in course of forty-eight hours; but it requires to be continued for *quite a fortnight after entire cessation of discharge*, to make sure the latter does not return. It produces these effects in the most acute and the most chronic cases alike.

As happens with most other remedies, however, there is a residuum of cases where it does not act so promptly, and in these cases there is no use continuing it, at least for the time. If within forty-eight hours the "running" is not arrested, another drug should be tried or therapeutic procedure adopted.

The difficulties in the way of arriving at a wide generalization as to the value of any method of treatment of the disease in question arise from the patient's not being under continuous observation, very often being only once seen and result not obtained. Then there is the *post-hoc-propter-hoc fallacy.*

Two cases under treatment within the last two years in my practice are selected as indicating better than most others the use and the uselessness of ol. sant. flav.

No. 1 with a gleet, which he said he had had for eighteen months. He had been to various medical men, and himself tried numerous remedies, but all to no effect. I thought it useless therefore to go over the routine remedies, and so passed a large bougie smeared with a liniment of vaseline and ol. sant. flav., and repeated this every third day for some time. He had also a mixture of steel and cantharides. After a reasonable trial of this line of treatment, and no improvement, the silver nitrate bougies were tried, one being used night and morning, in conjunction with a mixture of henbane and potash; the bowels being also regulated. At the end of two months he was in *statu quo.* I then at once put him on twenty-drop doses oil of santal three times daily before food. On the third day of this treatment he returned, saying that the discharge had entirely ceased, and asking why I had not given him that remedy at first! Of course I reminded him of what he had told me when he first came under treatment, and he was satisfied.

The same patient applied to me two years after, having unfortunately got a fresh gonorrhea. He was placed on a santal mixture, which restrained the discharge at once, and stopped it entirely within a week. He continued the mixture, however, for a fortnight thereafter, when, thinking himself well, having been perfectly dry for more than ten days, he omitted it. The discharge returned, and the ol. sant. flav. failed entirely to check it as it had done previously. Recourse was now had to various combinations of copaiba, cubebs, steel, and cantharides, but still the "running" was unchecked. Injections also had a fair trial, but seemed to make matters worse. Some months after, at the patient's own request, a recurrence was made to the ol. sant. flav. The discharge was at once arrested, a modification being noted after the first dose. The drug was, however, persevered with for two weeks, in order to make sure of the cure. At the end of this time it was stopped, but the discharge again appeared, only faintly. The remedy was therefore again used, and bougies smeared with it were passed every second day for another fortnight, at end of which time all medication ceased and the patient continued well.

No. 2 came suffering from retention. He had been under treatment for some time, and had been using injections. The "running" was unchecked, and he had made no water, save in dribbles, for thirty-six hours. I passed a catheter and drew off rather more than a pint of urine. The cause of retention was an inflamed and enlarged prostate. Next day he had swollen testicle and cystitis, and as these began to ameliorate the other testicle became inflamed. During treatment for these complications the "running," which was most profuse, was not heeded. As soon as the complications were subdued, however, he had fifteen drops of ol. sant. flav. every six hours, with the effect of checking the discharge and arresting it in forty-eight hours. On the fifth day the discharge was gone. When the patient got about again, however, and reduced the dose of the oil, it reappeared again, disappearing when he took larger doses. Various other drugs and injections and bougies have been tried, but the "running" continues. If he takes the oil in fifteen-drop doses thrice daily for three days he can reduce the discharge almost to nil, but when he leaves off it begins again as fresh as ever.

In by far the greater number of cases, nevertheless, the discharge does not reappear, provided the remedy is continued without intermittence and in full dose for a fortnight after the very last appearance. The average duration of cases treated by this method may therefore be broadly stated as three weeks. Twenty drops is a full dose, as this quantity invariably produces griping of the bowels and dull lumbar aching.

Chloralated Tincture of Iodine.—The tincture of iodine stands foremost in the list of immediate external coagulants and remedies provoking adhesive inflammation in closed cavities. Injections of iodine may bring about a cure by first as well as by second intention. The former takes place when cure results from one injection, as in ascites, hydrocele, etc.; the second when several injections are necessary, as in atheroma and cold abscesses. Carlo Pavesi (Lo Spallanzi), to further increase the therapeutical powers of the tincture of iodine, adds to it chloral, which dissolves in it without decomposition. The resulting preparation is miscible with water without precipitation. The proportion of its ingredients are: Iodine (very pure), twenty parts; chloral hydrate, thirty parts; spirits of wine (strength 36),

one hundred and forty parts. Mix, filter, and keep in an emery-polished bottle. The liquid is of a golden color, soluble in water, and has an odor and taste which indicate its ingredients. The chloralated tincture of iodine, on account of its markedly coagulating albumen, is an excellent hemostatic, and Dr. Pavesi considers it also very useful as an antiseptic and hypnotic.

CASE OF CROUP TREATED BY PASSING CATHETERS INTO THE TRACHEA BY THE MOUTH.—Dr. Wilson Paton, M.D., M.R.C.S., writes, in British Medical Journal:

H. J., aged three years and ten months, had measles, the rash appearing on February 15, 1881. On the disappearance of the rash a hard cough supervened, which gradually increased in severity until March 1st. On that date I found him, at 1:30 A.M., suffering from intense dyspnea, quite unable to speak, and his lips of a dark livid color. His cough was constant, brassy, and without expectoration. The respirations were thirty-five per minute, the cartilages of the ribs and sternum being drawn in at every effort to breathe, and crepitation existing over both lungs. The fauces were healthy. The pulse was 144, very weak. Having a No. 11 prostatic catheter with me, I determined to pass it into the trachea, instead of performing tracheotomy. Watching an opportunity, while the tongue was depressed with a spoon, the catheter, curved a little more than usual, was passed into the trachea during an attempted inspiration and without the slightest difficulty. A severe struggle followed, lasting perhaps a minute or two, the face becoming purple and the eyes staring with fully-dilated pupils. The paroxysmal efforts to expel the tube being unsuccessful, a pretty full inspiration, partly through the tube and partly through the larynx, followed. About two ounces of frothy, bloody, and purulent mucus were ejected by the tube and the mouth, the livid color disappeared, and he lay down, breathing easily through the tube. The presence of the tube did not prevent his swallowing milk, though sometimes a little of this was ejected from it during a cough. The tube was retained *in situ* by a strip of plaster, and the teeth were prevented from closing on it by means of a pear-shaped piece of hard wood.

Six hours afterward he was much easier, and could say "yes" and "no" distinctly. The cough continued at intervals of ten minutes, and did not seem altered in character by the presence of the tube. Crepitation still existed over both lungs, an abundant muco-purulent

secretion passing both by the tube and the mouth. Hitherto he had been kept in a warm room, but now a bronchitis-kettle maintained a moist temperature at 70° F. The tube was removed without any inconvenience after it had been in the trachea for eleven hours, as he had bitten it, and no air was passing through it. Shortly after its removal symptoms of obstruction gradually reappeared. During the same evening another ordinary gum-elastic catheter No. 12 was introduced, a slight momentary struggle and cough supervening. The presence of the tube led again to a very free expectoration of mucus. In the course of a few hours the respirations and pulse became lower, and crepitation and dyspnea ceased. When the tube had been in for forty-eight hours and a half it was removed and not again introduced. On March 8th the voice and chest-sounds were normal, and he was not seen after the 10th.

This case was a severe one, and would have soon ended fatally had no operation been performed. Tracheotomy seemed inadmissible, neither the case nor the surroundings being favorable for it. *Prima facie* it would be expected that the introduction of a tube into the trachea of a child against its will would not be so easy as in a consenting adult. That may be so; but it is certain that the operation is extremely easy and simple, and does not take more than two or three seconds from touching the tongue with the spoon till the tube is in the trachea. Had tracheotomy been performed successfully, when would the child have been out of danger? Certainly not so soon as here recorded; for at the end of the third day the child was so well as to be able to breathe freely without the tube, and was quite well before the tenth day after the operation.

Notes and Queries.

A CORRECTION.—In a letter from Dr. Keith concerning the article on Ovariotomy in the November number of AMERICAN PRACTITIONER he says, "The only really essential mistake is in speaking of the cautery at page 271, eleventh line from bottom. 'Parts will *not* slough,' it should read."

HYPNOTISM—DR. BEARD—DR. HAMMOND—DR. J. CRICHTON BROWNE.—It is pretty generally known that Dr. Beard went to England last summer and took with him a trained subject to exhibit the phenomena of artificial trance, of which he and Dr. Hammond are the principal champions in America. It is equally well known that Dr. Beard came to grief at his first seance in London and returned home. The audience before whom he appeared contained among others Bucknill, Horatio Donken, and Crichton Browne. Such a company was not long in discovering that Dr. B.'s trained man was a fraud—an imposter. They so expressed themselves to Dr. Beard, but respectfully coupled the expression with the remark that they believed Dr. B. himself was the innocent victim of the arrant knave. Dr. Beard was here afforded an opportunity to confess that he had been imposed on and to clear his skirts of all complicity with the rascal he had under charge. But Dr. Beard preferred another course. He denied that his subject was a fraud. He asserted that he himself was not deceived, and—rushed into the London newspapers. From there Dr. Beard subsequently transferred his guns to the pages of the New York Medical Record, from which, under cover of the heavy ordnance of his ally, Dr. Hammond, he has been firing at long range at the people who so effectually disposed last August in London of that hypnotism of which Dr. B. was the self-constituted and most windy prophet.

The latest phase of the controversy is contained in the Medical Record for January 7th in a letter from Dr. Crichton Browne, one of the very ablest men in Great Britain. We wish our space permitted us to copy the letter entire, for it is about the most thorough piece of work of its kind we have seen in many a day. We make room for a few extracts only, and those touching general rather than personal matters.

Hypnotism is a name covering some real facts and an infinite deal of falsehood. I do not pretend to know what has been done or not done in the name of hypnotism by Dr. Hammond in New York (his professional brethren on the spot are best able to judge of his experiments there), but this I do know, that the phenomena of hypnotism submitted to the medical profession in this country by the physician whom Dr. Hammond indicates as its most profound student and accomplished exponent were transparent humbug. If Dr. Hammond can give no better proofs of the faith that is in him than those adduced by Dr. Beard, I shall be compelled to believe that his laborious inquiries into hypnotism are analogous to the Scotch Hallowe'en pastime of winnowing three weights of nothing in the hope of seeing an illusion. He and Dr. Beard have yet to prove that they possess the most elementary and essential qualification for a successful student of hypnotism—a power of weighing evidence in relation to such obscure questions and of discriminating between chicanery and plaindealing. Until they repudiate some of their ridiculous heresies, such as the belief that a hypnotized person with his eyes securely bandaged can read a book through the skin of his forehead, it is almost a waste of time and loss of dignity for any man of science to argue with them on this topic.

I went to the meeting with no unfriendly feeling toward Dr. Beard, for I had had some pleasant intercourse with him at Cambridge, and had been favorably impressed by the suggestiveness of such of his writings as I had read, and with no conception that it would be requisite to make a stand for scientific truth. My suspicions were first aroused by a hint from Dr. Horatio Donkin, who had arrived before me, and whose quick insight had at once perceived the true nature of the performance with which we were to be favored; but it was what actually took place at the meeting that animated me, as it did many more who were present, to offer unflinching resistance to mystification when brought forward as new revelations in the science of psychology. Preconceived notions did not influence our action. Preparation for

the *exposé* there was none. Had we had an inkling of what was to take place, and five minutes in which to arrange our plans, the demolition of Dr. Beard's hypnotism would have been more final and complete than it was. Perhaps, however, there is no cause to repine. On the spur of the moment a wound was inflicted on it which, like Mercutio's wound, although not "so deep as a well nor so wide as a church-door," will prove "enough—'t will serve."

It is an ungracious task to refute error and unmask deception, but it is a needful one in these days, and one peculiarly incumbent on those practitioners of medicine who love and honor their profession and desire to secure for it a large measure of public usefulness and respect. In no way can the profession of medicine be more surely degraded than for those who follow it to exhibit a childish credulity or countenance the arts of the charlatan. Empiricism is the sin that doth most easily beset it, and it behooves the profession, therefore, jealously to guard itself against any teachings or practices that tend in that direction. That the teachings and practices of hypnotism may have that tendency will scarcely be gainsaid, and hence the duty imposed on the medical profession to keep a vigilant eye on hypnotic manifestations, and to repudiate at once any sympathy or connection with the vagaries and cheats and quackeries that are apt to gather round the small central nucleus of established hypnotic facts. Ample toleration must be shown to the widest diversities of honest and intelligent belief, but no quarter must be given to the delusions of the weak-minded, the whims of the superstitious, unworthy pretensions of those who are in too great haste to grow rich.

THE INDEX MEDICUS.—For three years now this publication has chronicled the current medical literature of the day. It has grown into a necessity to every real student in the profession in America. It can not hereafter be dispensed with. Yet it languishes for the lack of subscribers. Permit us to urge every one of our readers to assist in keeping this, the most important medical publication of the day, alive by sending to F. Leypoldt, publisher, 13 and 15, Park Row, New York, six dollars, the subscription price for 1882.

HALF KNOWLEDGE.—Our American atmosphere is vocal with the flippant loquacity of half knowledge. We must accept whatever good can be got out of it, and keep it under as we do sorrel

and mullein and witch-grass, by enriching the soil and sowing good seed in plenty, by good teaching and good books rather than by wasting our time in talking against it. Half knowledge dreads nothing but whole knowledge.

DR. MCMURTRY.—When it became known to the members of the Boyle County Medical Society that Dr. McMurtry had decided to remove from Danville they passed the following very flattering resolution:

Resolved, That the society express to Dr. McMurtry its high appreciation of his zeal and industry in promoting its welfare and advancing the interests and usefulness of the profession; that we part with him with feelings of unfeigned regret, and tender him our wishes for his success in the wider field of usefulness upon which he is about to enter; that we commend him to the regard and confidence of the profession in his new home, and especially to the Kentucky School of Medicine, which we desire to congratulate on securing him as a member of its corps of teachers.

The society concluded the well-merited compliment by electing Dr. McMurtry to honorary membership.

MRS. ALICE HART, the wife of the very able editor of the British Medical Journal, has pursued her medical studies with great zeal and well-merited distinction in the University of Paris. We took occasion now a year ago to praise some work she had done in connection with a study of the blood. Recently at the Academy of Medicine at Paris Dr. Noel Gueneau de Mussy presented and summarized two papers which were, he said, specially interesting as being the work of an English lady. The papers were on the number, size, and color of the blood-corpuscles in health and disease, and the symptomalogical value of the indications to be derived from this method of observation. Professor Gueneau de Mussy highly eulogized the matter and critical method of these papers, as showing a true scientific spirit.

I HAVE known a practitioner—perhaps more than one—who was as much under the dominant influence of the last article he had read in his favorite medical journal as a milliner under the

sway of the last fashion-plate. The difference between green and seasoned knowledge is very great, and such practitioners never hold long enough to any of their knowledge to have it get seasoned. (O. W. Holmes.)

THE AMERICA OF THE FUTURE.—We need in this country not only the scholar, but the *virtuoso*, who hoards the treasures which he loves, it may be chiefly for their rarity and because others who know more than he does of their value set a high price upon them. As the wine of old vintages is gently decanted out of its cobwebbed bottles with their rotten corks into clean, new receptacles, so the wealth of the new world is quietly emptying many of the libraries and galleries of the old world into its newly-formed collections and newly-raised edifices. And this process must go on in an accelerating ratio. No Englishman will be offended if I say that before the New Zealander takes his stand on a broken arch of London bridge to sketch the ruins of St. Paul's in the midst of a vast solitude the treasures of the British Museum will have found a new shelter in the halls of New York or Boston. No Catholic will think hardly of my saying that before the Coliseum falls, and with it the imperial city, whose doom prophecy has linked with that of the almost eternal amphitheater, the marble, the bronzes, the paintings, the manuscripts of the Vatican will have left the shores of the Tiber for those of the Potomac, the Hudson, the Mississippi, or the Sacramento. (*Ibid.*)

THE THOROUGH-BRED PHYSICIAN.—A physician of common sense without erudition is better than a learned one without common sense, but the thorough master of his profession must have learning added to his natural gifts. (*Ibid.*)